THE
AMERICAN
PSYCHOLOGIST

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THE AMERICAN PSYCHOLOGIST

The Professional Journal of the American Psychological Association, Inc.

In this Issue

OFFICERS OF THE AMERICAN PSYCHOLOGICAL ASSOCIATION

PUBLICATIONS OF THE AMERICAN PSYCHOLOGICAL ASSOCIATION

TRAINING DEVICES AND SIMULATORS:
SOME RESEARCH ISSUES[1]

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RESEARCH on training devices is an area peculiarly fitted to the talents of the experimental psychologist, first, because he knows the field of learning, and second, because he usually likes to tinker with gadgets. The combination of these interests and skills has not only made training devices research attractive to many psychologists, but has also created a demand for their services. This has been true for a number of years, in fact, since World War II when training devices were used widely in the armed forces. And this interest and demand have led to the establishment of a number of agencies and programs in which the psychologist participates in the design, development, and evaluation of training devices. The Army, Navy, and Air Force all have research programs in these areas.

Many kinds of research activity are evident from published reports. Probably a great many changes, presumably desirable ones, have been made in the design of training devices as a result of psychologists' participation in decisions about design characteristics. There are also a number of studies on the "effectiveness" of training devices (e.g., **13, 24, 30**) which are generally characterized by sound but unstartling conclusions. The discussion of research problems by Wolfle (**31**) has remained the major source of systematic thinking for several years. Nevertheless, all these things do not constitute the kind of effort, nor the kind of scientific product which might be expected from research having such a high degree of intrinsic interest and so many ramifications into nearly all fields of psychology.

If one rejects a narrowly applied point of view, one can see that a considerable variety of research

questions are relevant. Perhaps first in importance is the ubiquitous problem of how to measure complex human performances. The "criterion problem" has been with us for a long time. It has been solved in specific instances either by arbitrary determination or by selection on the basis of empirical data. But the need for a fundamental solution which seeks to account for the variances attributable to machine and man, and the relationships between behavioral processes and products, is pointed up clearly by the training device.

Bound up with the criterion problem is, of course, the perplexing methodological question of how to analyze job activities. For the kinds of performances to which most training devices relate, it is apparent that the methods of traditional job analysis are not particularly helpful. Yet the necessity of differentiating the critical activities from those which are routine and easy is one of the central problems actually faced by anyone who designs a training device, whether he is a psychologist or not. Since in many situations a training device functions as a performance test, there is a need for the application of the principles and methods of test construction, analysis, and scoring. In fact, many problems of training device use point to the development of new principles and methodology, including perhaps a fresh examination of the concepts of reliability and validity.

Some of the most intriguing research problems in the design and use of training devices are fundamental problems of human learning and retention. When one inquires about the effectiveness of the characteristics of a device for training, one is really asking about the transfer of learning to some criterion performance. When one inquires how a training device may be designed to yield highest transfer, one is actually asking more intensively about the conditions of efficient learning. Since human beings are highly verbal, it is not too surprising to find many of the characteristics that can

be manipulated in the design of a training device are those which may be presumed to change a self-instructional set, or a source of motivation. Other characteristics may determine the range of variety of responses practiced and the amount of information provided to the learner during practice. In other words, within the framework of training device research can be found some of the central problems of human learning.

SOME DEFINITIONS

It may be helpful at the outset to try to phrase some definitions and distinctions. Typically, there is a certain amount of disagreement over a terminology which comes partly from psychology and partly from engineering, with a little dash of administrative cliché thrown in for good measure. The following are working definitions and refer primarily to content of the present paper.

Training device. As usually understood, *training device* refers to any piece of apparatus which is used for training individuals. A somewhat more precise and useful meaning, also common, is that a training device is used for the training of skills. Such a definition serves to distinguish the class training devices from the class training aids, which are objects and devices used to facilitate the presentation and teaching of informational knowledge. In this more restrictive sense, training devices are characterized by the possession of a *display* which presents information necessary to the correct operation of *controls*, which the student must learn to operate, in most cases during periods of practice on the device.

There are many occasions in which the actual equipment, rather than a substitute device, is used as a training device. A simple example is the rifle. In addition, no hard and fast distinction can be drawn between what piece of equipment is a training device and what is a piece of operational equipment. For example, an airplane used for basic flying training can certainly be considered a training device; yet an additional ground trainer may be used to establish skills used in the training airplane. What distinguishes a training device is not its appearance or construction, but rather *how* and *for what purpose* it is used.

Simulator. A simulator is generally understood to be a kind of training device which has a high degree of resemblance to operational equipment, particularly with respect to the display, the controls, and the way one affects the other when in operation. For example, a simulator used in training for a particular aircraft is expected to be like the aircraft in cockpit arrangement, furnishings, lighting, instrument panel, and controls, as well as in the effects which control movements have upon instrument readings or upon the attitude of a movable cockpit. It is, of course, well known that perfect simulation is never completely achieved, and that the more closely it is approached, the greater the expense is likely to be. It will be suggested later that the clearest usefulness of a simulator is that of proficiency measurement.

Evaluation. The phrase "evaluation of training devices" has a number of meanings which can be confusing indeed when used interchangeably. The following brief descriptions of evaluative activities may help to sort out these meanings.

a. Engineering evaluation of a training device is usually done on the first manufactured model of a device when it is delivered to the agency which ordered it, for the purpose of determining whether the device meets required specifications, i.e., whether specified physical tolerance limits are met. This evaluation is usually done by engineers.

b. Field evaluation. Just as is the case with a new weapon, it is reasonable that every new training device should have a tryout or evaluation "in the field." Many questions may be answered by such an evaluation, including those of acceptability of the device to instructors and students, ease of maintenance, facility of operation, and reliability of operation. Such questions are important, even though they can often be answered by relatively simple observation or by the collection of records over the interval of time when the device is in use. However, it would be a mistake to suppose that such data alone can yield any positive information about the effectiveness of the device for establishing desired skills.

The crucial question in a field evaluation is that of *training effectiveness*. The *training effectiveness* of any device for a particular job is the difference between measured performance on the job preceded by practice on the device, and measured performance on the job not preceded by practice on the device. This is the kind of evaluation for which there is a clear application of the technique of the controlled learning experiment and the measurement of transfer of training. It is often the most difficult experiment, either because of lack of a

criterion performance measure or, frequently, for administrative reasons. It is reasonable to expect, though, that a psychologist's activities will always be based upon the knowledge that there is a clear-cut experimental method and a definite numerical answer to the question, "How effective is x hours of practice on device A in establishing the skills required in operation of equipment B?"

PERFORMANCE IMPROVEMENT VS. PERFORMANCE MEASUREMENT

When one looks at the situations in which training devices and simulators are used, one fact emerges with great forcefulness: these devices are used very frequently and extensively for the *measurement* of performance, as opposed to the improvement of performance through practice. A typical procedure in the use of instrument flying trainers, for example, is to require the student to "fly" standard missions with navigational and instrument "problems" which tax his knowledge and ingenuity. Another procedure consists in the systematic introduction of various failures in equipment which call upon the student to use emergency procedures. Now, it cannot be said that these procedures are designed to provide the kind of practice that is typical of actual aerial flight, because a great many more happenings of a critical nature are crowded into a ground trainer mission than would occur in the air. Instead, these procedures usually reflect a desire to "see what the student can do." In practice, this may be carried out by the instructor's observing how well the student performs in terms of some over-all judgment, by determining the limit of what the student can "take" in number of emergencies, or by some objective recording of his performance. However it is done, the aim is to measure proficiency.

To maintain that no training is accomplished when devices and simulators are used in this way would, of course, be incorrect. The important point, however, is that the procedures employed are usually not intended to give systematic practice, but primarily to provide hurdles or problems which sample the repertoire of knowledges and skills, as any good measure of proficiency is expected to do. There are many situations in which devices are employed in measuring proficiency, though this use is not always recognized or stated.

On the other hand, it is quite possible to separate the function of *performance improvement* from

that of *performance measurement*. A device may be, and frequently is, designed for the sole purpose of providing practice in essential skills. Here it is obviously not essential for the device to possess a scoring system for the measurement of performance. There are many relatively simple devices in which the student is simply run through a series of exercises by an instructor, with perhaps some accompanying qualitative judgments about the student's improvement with practice. This type of device, though inelegant and perhaps inexpensive, can possess potentialities for performance improvement equal to those of more elaborately constructed devices which yield precise scores. Furthermore, its training effectiveness can as readily be evaluated by means of a transfer experiment.

Thus it is possible to conceive of a device which is primarily designed to measure performance, or one which is designed solely to provide opportunity for learning of critical skills. The two functions may or may not be present in the same piece of equipment. But the important point of the distinction is this: *Performance improvement may require different characteristics for effectiveness than does performance measurement*. For example, reliability of scores obtained from a device is relatively unimportant for improvement, but essential for measurement. Transfer of training from practice is the essential criterion for performance improvement, but obviously irrelevant to measurement.

This distinction also serves to put the question of simulation in its proper place—which is not conceived to be one of primary emphasis. According to this idea there are two essential questions: What characteristics of a device are essential for performance improvement? And what characteristics are essential for performance measurement? Within such a framework, simulation becomes merely one of many factors to be considered. It is important for training to the extent that it implies adequate representation of critical skills, but only to the extent that it does not impede the learning of these skills. It is important for performance measurement to the extent that it makes for validity, but only to the extent that it does not interfere with adequate reliability of measurement. A training device, in other words, cannot be justified on the grounds that it looks good (i.e., closely simulates an operational situation). It must do something, too, and that something is either training or meas-

urement. Simulation becomes a secondary matter whenever it conflicts with these purposes.

Whether a device will be used for performance improvement or for performance measurement, or both, is an extremely important question which affects each and every stage in the life of a training device, from its initial planning through the various stages of development, evaluation, and use. It is also this question, we believe, which can bring the maximum of order to the planning and conduct of research programs.

RESEARCH ON TRAINING DEVICE DEVELOPMENT AND USE

What kinds of problems are generated when consideration is given to the design of a training device (or simulator) or to its use in a training program? These problems arise when decisions are made concerning what to build, as opposed to considerations of testing what is already built. It is reasonable that the first stage of this process is to determine what the job is like for which a training device is required. This involves some kind of *analysis of the job,* with the emphasis being placed upon the activities or behaviors present. It is also important that a device's characteristics necessary for adequate *performance measurement* be given separate consideration from those characteristics which make for *performance improvement.*

Analysis of the Task

Since the building of a training device or simulator is undertaken in the first place because of a need to represent the actual job (either more simply, less expensively, with smaller involvement of danger to the operator, etc.), it is obvious that some decisions must be made, at the very beginning, about what are the essential aspects of the job to represent. Although such decisions are always made when a training device is designed, they are not always made in a systematic manner. And sometimes they seem to be perverted, rather than clarified, by attempts to follow the principle: "Make the device as nearly like the actual equipment as possible."

In general terms, the way to go about describing and analyzing the job to gain knowledge of the requirements for training device design would seem to be to categorize the kinds of specific equipment-oriented behaviors and skills which are involved. To use a relatively simple example of an operator's

job, flexible gunnery, it has been recognized for many years that some of the essential behaviors are tracking, ranging, and triggering (8, 9). The significance of this categorization of component behaviors may be very simply stated. It is impossible to design an adequate trainer for flexible gunnery unless one knows that these behaviors are, in fact, important in the job. The trainer is built, not to represent some abstraction called "flexible gunnery," but to create a situation in which these activities can be practiced on the ground under conditions leading to improvement in their performance in the air.

Some idea of the process of deciding what behaviors and skills are essential was recently gained by the writer when he participated in preliminary discussions of a projected helicopter trainer. The student helicopter pilot must, of course, learn to do many things. He must learn how to preflight the equipment; how to take off; how to coordinate rotor and engine speed; how to fly forward, backward, to the right, and to the left; how to correct for torque; how to hover; how to land; what to do in emergencies; and numerous other details. The first and most obvious point made in discussion was simply this: If one determined to build a trainer to represent all these behaviors, it would not be a trainer, but a helicopter. It became evident that what had to be decided was what were the critical skills which ground-trainer practice could be expected to facilitate. There was practically universal agreement among training experts that the most critical behavior was hovering, and the agreement on this human activity strikingly brought order and direction into the discussion. One could now speak of a "hovering-trainer," rather than a more abstract "helicopter-trainer"; the operating characteristics needed for such a trainer then became relatively easy to formulate.

What can the psychologist contribute at this stage of the process? First of all, he can insist that decisions about what aspects of the job a training device represents are arrived at by systematic consideration of operator activities, rather than by consideration of the equipment alone. Further, he can use his knowledge of human behavior to analyze and describe the categories of behavior which a job contains. But though he may do this, and perhaps succeed at it better than other specialists, it is this activity that makes him most keenly aware of the limitations of psychological knowledge.

What, in fact, are the categories of human behavior? Or even, of human equipment-operation behavior? This is one of the most challenging areas of research for the experimental psychologist. Few attempts have been made to bring scientific order into this field.[3] Although a kind of taxonomy must probably be involved, the important research problem appears to be the development of a theoretical system which will relate physical task variables to performance variables by means of conceptualized intervening processes. The lack of such a theory creates a void in this area of human behavioral knowledge. When he has developed such a theory, the psychologist will be able to say in what exact ways flying forward in a helicopter is like or unlike hovering, or taking off, or flying backwards, or landing; or, for that matter, is like or unlike flying forward in a conventional airplane, or driving forward in an automobile. At that point he will be able to state with considerable precision what behaviors should be represented in a training device.

The question of how closely a device should be made to simulate an operational situation can often be reduced to the question of critical skills. For example, if a flying trainer is designed to train the critical skill of landing, it is known that certain types of instruments are just not used in the practice of this skill, i.e., they provide no stimuli to which the learner must acquire appropriate responses. On the other hand, if flying an instrument navigation mission is the critical skill for which training is desired, then clearly some of these same instruments may be essential, although visual stimuli from the terrain are quite unnecessary. In many instances, the discrimination between what is essential or unessential for the representation of a critical skill requires no great amount of technical knowledge. Nevertheless, there may be considerable need for emphasis on the *purpose* to be served by a training device. When a need for a training device is developed, this in itself implies the belief that certain essential skills can be more simply represented than by means of a replication of the operational situation. Unless this purpose is constantly borne in mind throughout the development process, the demand for close simulation can readily lead one back to the operational situation itself. This is no solution.

[3] Two important exceptions are articles by Brown and Jenkins (10) and by Craig and Ellson (12).

Performance Measurement

If one accepts the purpose of performance measurement as a legitimate function of a simulator, desirable characteristics may be defined with considerable clarity. Any test or measure of performance should aim for high *validity*, i.e., some sort of assurance that the test measures what it is supposed to measure. When one mentions measurement, though, reliability as well as validity is implied, since it must also be determined that the test yields a performance score which differentiates between a superior and inferior individual with some degree of dependability. To the psychologist, these are well-known concepts. They appear to be applicable without change or reservation to the measurement of performance by means of a training device. Their application in this new field gives rise to research questions which have been with us for many years and serves to emphasize these problems in particularly vivid fashion. Specifically, these are questions concerning the nature of criterion measurement.

Validity. It is apparent that close simulation has the aim of insuring high validity to the task presented by the trainer. It is not quite as easy as it sounds, though, to produce a highly valid measure of performance by exact simulation of the operational equipment. The chief reason is that there are some very difficult unsolved problems about performance criteria (cf. 29), all concerned with what aspect of performance shall be measured. For example, what is the ultimate criterion of an aerial combat gunner's performance? Is it number of planes shot down? Number of planes hit? Number of planes scared away? And should friendly and enemy planes be distinguished from one another in these frequency counts? It should be pointed out here that these questions may be legitimately asked independently of the question of reliability. They are questions concerning the meaning of criteria and the relationships between products of behavior and measures of the behavior itself. They represent an area of psychological knowledge which is not highly developed. And, obviously, they imply that the problem of simulation cannot be adequately solved without facing the more general problem of criterion performance measurement. Only when he knows some general rules about the relation of performance to product will the psychologist be able to predict with con-

fidence what performances should be simulated to insure high validity.

In many industrial activities, the adoption of a product measure such as "number of units assembled" seems an entirely reasonable performance criterion. But there are many other human activities, both within and without the armed services, to which the application of such a measure appears impossible. There are others to which some form of product measure might be applied. For example, one can conceive of using "number of hits" as a criterion of performance in flexible gunnery during a specified number of aerial missions. What is usually employed is some more analytical behavior measure such as "miss distance." There is little evidence, and no theory, which enables a choice of the most desirable criterion measure, except on the basis of reliability.

Reliability. Even assuming that high degrees of simulation are desirable for validity, sacrifices at the expense of similarity must often be made for the sake of reliability of performance measurement. The position of the psychologist in this matter is, presumably, that regardless of the possibility of greater validity with high similarity, no measurement of performance is possible without some degree of reliability.

It is instructive to note here some studies which have investigated the measurement of student pilot performance in aerial missions.[4] In these studies the aerial task was designed to simulate exactly the maneuver taught; in fact, they were identical. Moreover, the observations of behavior were made as free from the likelihood of human error as possible, in order to remove this source of unreliability. Nevertheless, the results showed many of these performance measures to be quite unreliable—not because of observer error, but because the performance of the individual student varied from one time to the next. In this instance, regardless of their admitted validity, the degree of unreliability in these measures imposes a serious obstacle to their use. A similar conclusion would be drawn were these results obtained in a ground trainer which closely simulated aerial maneuvers.

There is at least one way in which a training device may have to be dissimilar from an operational

task in order for reliability to be achieved. The device must be capable of providing an adequate sampling of the particular behavior to be measured. Stated in terms of analogy to a proficiency test, the reliability of a device will increase as the number of items measuring a given type of activity is increased. This principle is utilized in many present simulators, though in a somewhat unsystematic way. For example, aircraft simulators often provide for the running of "missions" which include a number of different instances of simulated equipment failure requiring the use of emergency procedures. Repetitions of these emergency situations are provided, not because they occur in an operational situation, but (at least partially) because of the need to obtain a large enough sample of the student pilot's behavior to permit a reliable assessment of his performance.

Actually, there is room for a great deal of improvement in simulators in the application of this principle of adequate behavior sampling. If the purpose of a training device is performance measurement, there must be increased acceptance of the fact that simulation has to be diluted to the extent that it is necessary to provide long enough, or large enough, or frequent enough activity sequences for performance to be measured with minimally acceptable reliability.

Research in this area should have the aim of formulating a set of principles relevant to the arrangement of conditions in a trainer situation so as to produce maximum reliability of performance measurement with the sacrifice of as little validity as possible. The general question is: How must the characteristics of an operational task be deliberately altered in order to make possible adequately reliable measurement of performance highly related to the operational task?

One problem in training device design which arises frequently is the degree of accuracy which must be specified for a system of scoring performance. Accuracy refers to repeatability of the limits of the scoring area; it should be clearly distinguished from accuracy in the sense of deviation of scoring area from some absolute physical value. Obviously, the greater the accuracy in the first sense, the better. Minimal acceptable accuracy must be estimated in terms of the relation between the amount of variance contributed by the device and the amount of variance contributed by the inherently variable human operator. Up to the pres-

ent time, only by collecting empirical data on each particular performance which is being scored has it been possible to estimate the minimal acceptable apparatus accuracy. Is it possible that data on the variability of human motor activities could be cast in a systematic framework? Is it possible, for example, that one might eventually be able to predict the variability of motor response functions as well as is now done for verbal intelligence? The exploration of this area of human engineering appears a very worth-while research effort.

Investigation of how the length and arrangement of performance samples affect reliability is also needed. The construction of items for printed proficiency tests is an activity which can now be carried out on the basis of a number of verified principles pertaining to such matters as length of items, number of misleads, arrangement of items, and so on (cf. 1). To what extent can these principles be translated to a performance-measuring situation, in which the behavior to be measured is to a greater extent controlled by the apparatus, rather than being determined by the experimenter? How long should a single trial be for different types of responses? How can one arrange trials to yield a homogeneous proficiency measure in spite of changes in performance with practice? What is the relation between length of the behavior sample and reliability? Is it possible to arrive at generalizable principles of motor performance measurement which will apply to the great variety of human operator activities, so that the necessity for empirical determination of each case can be avoided?

Effectiveness for Training

If facilitation of performance improvement is the purpose being considered for a training device, a desirable aim of research is the determination of what characteristics of the trainer task will increase transfer of learning to the operational task. More specifically, research should tell us how the physical characteristics of a trainer may be designed to bring about the most rapid acquisition and the highest possible level of performance in the operational skills for which training is required. The question can even be expressed in this way: How must the characteristics of an operational task be deliberately altered in order to insure the most effective training by means of a training device?

This point of view may be exemplified in its broad aspects by considering the implications of an experiment by Lindahl (23). This investigator was concerned with the problem of effective training for the industrial job of disc cutting, in which discs must be cut from a tungsten rod by means of an abrasive wheel operated with a foot pedal. The standard training situation, with "exact simulation," would have been to provide new workers with regular periods of practice on the disc-cutting machine itself. This type of practice did not appear to Lindahl to be necessarily most effective. Instead, following an analysis of the foot movements involved, he arranged for new workers to practice a standard pattern of foot action in response to a visually presented model. Comparison of production records of new employees trained by this method with those of older employees who had simply practiced disc cutting itself showed that the former achieved a level of performance after 11 weeks of training equivalent to the level of those who had been on the job for 5 months.

Wolfle (32) considers that this experiment illustrates the effectiveness of a training method which emphasizes "process" rather than "product." Other possible conceptions may suggest themselves. The most important implication, however, is this: A training situation which was deliberately changed from a job situation in order to emphasize critical aspects showed transfer to the final task greater than that produced by practice in the job situation itself. More effective training resulted when "exact simulation" was sacrificed.

This example does not necessarily violate the hallowed principle that amount of transfer between tasks increases with the number of identical elements. Indeed, it may be said that transfer was high in this situation because the foot movements practiced were identical with those of the most highly skilled disc-cutter operators. The problem of effective training, however, was not one of making the tasks similar, but rather of arranging the conditions of practice in such a way that essential skills were most efficiently learned. One may generalize from this example that the answer to the problem of what makes a training device effective is to be sought, not in identity of all task elements, but rather in viewing a training device as a means of making conditions most effective for learning.

There are many ways of arranging the physical task or conditions of practice, each of which may be viewed as a deliberate alteration from the conditions of exact simulation, and any or all of which

might conceivably have the effect of facilitating the learning of operational skill. Suggestions for these arrangements come from a variety of research sources. Since none of the results can at present provide definite answers to the problem of facilitation of skill acquisition, they have the status at present of questions, more or less adequately defined, which research can be designed to illuminate. Some of these possibilities are discussed briefly in the following paragraphs.

Repeated practice. One essential condition for the learning of skill is, of course, practice. As has been pointed out, the provision of opportunities for repeated practice is one of the most obvious advantages a training device can have, as contrasted with an operational situation. The motor skills in an airplane maneuver can be practiced many times during a relatively short session in a training device, while an equivalent amount of practice in a training airplane requires a very much greater expenditure of time and effort. In a good trainer, the task is deliberately designed so as to make possible the repeated practice of critical skills.

There are some unknown things, however, about even this essential variable of practice. Is there a general answer to the question, how much practice is desirable for most effective transfer? Or, in terms of experimental variables, what is the relation between amount of initial practice and amount of transfer? At the present time, little more than a qualitative answer can be given to this question, to the effect that transfer increases with amount of initial training. For many problems of training device design, it will be necessary to provide answers in terms of how much. The problem has a number of facets, but perhaps the most obvious one is the need for a scale of measurement for transfer.

Although some attempts have been made to bring order to this problem of measurement (cf. 15), there is still a great need for research. Perhaps the most crucial question is that of deciding what should be meant by the upper limit of transfer, particularly when what is learned is increased response accuracy, rather than a specific set of "new" responses.

Motivation and reinforcement. A training device may be designed to provide additional or different motivation from that of the operational task. Although such things as monetary rewards are not particularly relevant to the training device situation, a number of well-known forms of variation in competition and cooperation (cf. 26, pp. 470–499) appear to be. A recent study by Bilodeau (3) has explored one possible arrangement of the competitive situation in the learning of a motor skill. The negative findings of this study do not, of course, make additional research less desirable.

Knowledge of results, in its many forms, has often been considered a variable which has motivating properties in the sense of reinforcement. (Its information-giving properties will be considered in a subsequent paragraph.) In motor skill learning situations, studies (20, 25) have tended to show that telling the learner when he is on target or off target (comparable to saying "right" and "wrong") does not improve transfer, though it does have a fairly immediate effect on the level of performance. The results suggest that the added on-target knowledge becomes an extra cue for the establishment of certain responses which rapidly extinguish once the cue is removed. These results may have a decelerative effect on the tendency to make blanket recommendations for "knowledge of results" features in training devices. However, it can certainly not be maintained that the full range of reinforcement possibilities of this variable has yet been explored.

Another variation in reinforcement suggested by conditioned response studies is the frequency of application. Under certain conditions, results of these studies suggest a heightened resistance to extinction of the response established under less than 100 per cent frequency of reinforcement (cf. 17). Can such a finding be extended to the learning of human motor skills? Up to now, several studies (6, 20, 25) of reduced frequency of presentation of knowledge of results, at regular trial-intervals, have failed to give any clear indication of learning facilitation or heightened transfer. However, regularity, as opposed to randomness of reinforcement, may be a critical condition of these findings in view of the possibility that the learner may quickly acquire habits of reaction to the regular patterning of on-target information. Thus, a potentially fruitful research area is the effect of random presentation of knowledge of results on the learning and transfer of motor skills. There are many varieties of frequency patterning of reinforcement, as the work of Skinner (28) shows, each having different consequences in the performance of simple acts. Under what circumstances can these variations affect the acquisition of complex skills?

Set. It is conceivable that an important result of the use of a training device might be the establishment of a set which could not be reliably produced under conditions of exact simulation. It is possible, for example, that habits of attending to critical task elements might later facilitate performance of an operational task. Or, the division of attention which is required of the pilot in high speed aircraft might be established by suitable arrangements of the presentation of stimuli on an instrument trainer panel. Our present knowledge of the conditions of establishment of such sets, and the ways in which they influence the learning and performance of skills, is very inadequate. A recent study of flexible gunnery skill (7) shows that an alteration of the physical characteristics of the sight can produce significant performance differences presumably because of changes induced in the subject's attentive habits. At least the potential fruitfulness of this research approach is supported by these results.

The possibility of establishment of a learning set, described by Harlow (19), also appears to be a worthy field for investigation. It seems possible that systematic variations in the stimulus material, which one study (27) showed to be effective for transfer, may be one way of establishing such sets. There may be other ways, and the use of verbal processes available to human operators, but not to Harlow's monkeys, should not be overlooked (cf. the discussion by Wolfle, 32, pp. 1272–1275). The potentialities of "learning set" for training human beings who will probably be faced with the need to make frequent adjustments to rapidly changing equipment appear great.

Component practice. It is doubtful that any complex skill is ever learned entirely by practice "on the total skill." Training in driving an automobile, for example, is very often conducted in sessions designed to give special emphasis, or additional practice, to such difficult part-skills as shifting gears or the maneuver of parallel parking. In this respect, practical skill-training methods have always departed more or less from the conditions of exact simulation.

If the complex skill for which training is desired may be analyzed logically into component activities which differ in difficulty, the research problem of the relation to total-skill performance of differential amounts of training can then be explored. For example, the task of B-29 gunnery sighting is usually described in terms of the components of tracking, framing, and triggering. Though no systematic study has been carried out, it is a reasonable hypothesis that differential treatment of these three components in terms of practice would yield differential amounts of transfer to the total task. The implication of such results might very well be that systematic arrangement of component practice produces greater transfer to complex skills than does a "natural" (i.e., highly simulative) arrangement. From a research point of view, this area may be expected to yield findings of considerable import for an understanding of how complex skills become integrated from simpler activities.[5]

Response precision. Much effort and expense are sometimes devoted to making the degree of tolerance permitted by a control movement in a training device highly similar to that of the operating equipment; for example, the design specifications for an instrument flying trainer may state that the ratio of a given stick movement to a given instrument-needle displacement must simulate that of the aircraft exactly. Yet the fact is, we know little about the effects of variation in "target tolerance" on the learning and transfer of skills. Some recent studies on this subject (4, 5, 14) strongly suggest that the generalization "transfer increases with similarity" is of limited usefulness indeed when applied to the characteristic of on-target response tolerance. Instead, they support the use of a somewhat simpler principle, "transfer of skill is unaffected by wide variations in target tolerance." The factors which do determine these limits remain to be discovered by additional research; but a simple principle of "similarity" does not appear to be relevant.

The most obvious effect of increasing the size of on-target tolerance in a training task is a marked increase in the rate of improvement in performance scores during practice. One hypothesis would regard such an increase in success as a motivating or reinforcing factor. As already mentioned, however, the evidence obtained so far does not support such a hypothesis.

[5] Research on component skill training bears only a slight resemblance to older studies of verbal part-whole learning, summarized by Hunter (21), or to the work of Batson (2) and Kao (22) on motor tasks. Generally, in this older work, interest centered on the learning of total *sequences* of part-activities, as opposed to the integration of component activities which must often be simultaneously exercised.

On the other hand, one might think that decreasing the target tolerance in a training task, despite making the task more difficult (in the sense that less success is achieved), would serve to encourage greater precision of responding during practice, i.e., prevent the occurrence of responses outside the desired operational tolerance range. This, in turn, might have the effect of facilitating transfer to an operational task. Things do not seem to work this way; at least one study (5) indicates that the range of responses actually practiced is unaffected by target tolerance.

A somewhat different method of restricting the target tolerance permitted in a training task is the use of what may be called *enforced guidance* (cf. the studies of Carr, 11). One can think of designing a training device which would physically restrict the range of control responses permitted, in order to make it impossible for the learner to practice "over-controlling" responses or the making of gross errors. What would be the effect on transfer of a training situation which permitted the practice of only "correct" responses, as opposed to one which allowed the making and correction of errors? This question seems to have rather interesting roots in basic learning theory, including particularly that of Guthrie (18).

Surely the last word has not yet been said on this problem. There are many ways and degrees in which target tolerance can be varied throughout a practice session, and there are many different theoretical reasons leading to the prediction of changes in both rate of acquisition and transfer of motor skills, as a result of such changes.

Performance feedback. It is not possible to distinguish this characteristic clearly from those discussed as knowledge of results and target tolerance, both of which include forms of performance information fed back to the learner. Nevertheless, there are some research hypotheses arising from a consideration of this characteristic as having only the function of information giving, as contrasted with the function of motivation or reward.

For example, what kind of information can best be utilized by the learner in the sense of producing greatest transfer? On a flexible gunnery trainer, one study found no transfer gains from an indicator that informed the learner when he was "on target" (6). On the other hand, a study conducted on a similar trainer (16) showed lasting (i.e., presumably transferable) skill changes when information

was given about the direction and extent of errors. As yet, these results may be considered only suggestive of a generalizable conclusion. But they indicate that a general research problem may perhaps be stated as that of discovering principles of efficient coding of feedback information obtained by a learner during practice on a training device.

Besides the problem of frequency of feedback, which may be considered from the information-giving point of view as well as in terms of the previously mentioned concept of reinforcement, there is the matter of the size of "chunks" of information. The usual procedure is to restrict the performance information to a report of the last preceding trial. But it is conceivable that a reporting of performance on a greater number of preceding trials, whether in serial or summarized form, might have some differential effectiveness for learning and transfer. In other words, this, or other methods of providing summarized feedback covering larger segments of performance, might be considered as another kind of variation in information coding. Systematic studies of the application of this concept of coding to feedback information have not yet appeared.

Summary. There are many ways in which the training device situation might be deliberately altered from the operational situation in order to serve the purpose of performance improvement. At the present time, we do not know that any of these changes would, in fact, produce transfer differences when compared with training conducted under conditions of close simulation. The problems offer a challenge to the research psychologist running somewhat as follows: "Can you discover principles which may be applied to training devices in such a way as to make them more efficient than haphazard on-the-job practice?" It should be emphasized, perhaps, that even if negative findings can be established for any or all of these factors, we shall be much farther along in our understanding of training device design requirements and shall also be able to make some money-saving recommendations.

EVALUATION OF TRAINING DEVICES

Once a training device has been constructed, it needs to be evaluated. The first type of evaluation should logically be an engineering effort to determine whether the physical specifications contracted for have been met by the manufacturer.

When the training device is put into use, there are a number of evaluative questions which are the legitimate concern of the experimental psychologist.

The aims of field evaluation research on training devices are, generally speaking, quite different from research on design principles. In the field use situation, one is concerned with answering questions about a device which is being put to more or less specific use in a particular course of training. The research question may take the general form: What are the conditions which determine the most effective use of this device, either for training, for performance measurement, or both? Research of this sort may suggest a number of design principles, though this is not its specific aim; and such principles, of course, cannot be applied to the particular device in question, since it is already built.

The distinction between the aims of performance measurement and performance improvement can serve to clarify greatly the approaches of field evaluation research. Certain characteristics are plainly irrelevant to the value of a device for training, just as others are irrelevant to the value of a device for performance measurement. When the psychologist is given the job of evaluating a training device, the first question he should answer is, "What is the purpose for which the device will be used?"

Evaluation as a performance-measuring device. The determination of reliability is of primary interest in a training device used for performance measurement. Studies are often required to determine more than the reliability of a single type of score alone. For example, the question of how long a behavior sample should be used or of how many trials of a particular type should be included in a measure are frequent research questions. The psychologist should be concerned with recommendations regarding the conditions under which performance measurement is to be accomplished, if it is to have the reliability essential to its usefulness in a training program.

In a field evaluation study, validity is determined in order to assess the degree of relationship between performance measures obtained in the device and performance involved in some criterion task. It should be borne in mind, on the one hand, that the purpose for which the validity coefficient is obtained is to evaluate the *device,* and not the criterion; though it is true, of course, that the size of the relationship may provide some information about the nature of the criterion. On the other hand, the limitations on the size of the validity coefficient imposed by the reliability of the criterion are of crucial importance for evaluating this relationship. One cannot determine the degree of resemblance of performance on a simulator to operational performance if the latter can only be measured with zero reliability. Since there are many instances of the latter situation at present, it would seem best frankly to face the fact that validity cannot be measured in these cases. If the training device yields reliable performance scores, it may be desirable to decide on rational grounds (cf. 29) to use the device itself as an intermediate criterion. But no amount of correlating and intercorrelating can by itself solve the problem of unreliability of the criterion.

It is not essential to the evaluation of a performance-measuring device to determine its learning and transfer characteristics. The device may measure performance in both a reliable and a valid manner without contributing to performance improvement. For example, relatively short single measurement sessions may yield adequately reliable scores which are highly related to criterion performance, without affecting the level of skill. Improvement in skill may require practice of considerably greater duration either outside or inside the training device situation. Conversely, indications of low transfer between the device and operational situations do not necessarily imply a low validity relationship, since the device may, and often does, *train* only a portion of the abilities exhibited in the criterion task.

Summarizing, the most important aspects of the evaluation of a device for performance measurement are the reliability of the measurement and the degree of its relationship to an operational performance criterion. Both are measured by a correlation coefficient. Measures of performance improvement, however, whether obtained in the trainer situation or in the operational situation, are irrelevant to this question.

Evaluation of training effectiveness. The primary concern of research designed to evaluate the training effectiveness of a device should be to obtain measures of the transfer of training on the device to an operational performance situation. Since a device seldom has one single effectiveness value, it is usually desirable to frame more analyti-

cal questions for research such as: At what stage in the course of training will practice on the device yield greatest transfer? Or, what amount or type of practice on the device produces maximal transfer to the operational task? Results of such studies may be expected to reveal, not a yes-no answer to the question of training effectiveness, but rather in what ways the device can be used so that it will be optimally effective.

Since reliable performance measures in operational tasks are frequently difficult to obtain, the problem of measuring transfer is also difficult. A partial answer to the question of training effectiveness may be obtained by measuring improvement in performance within the training device situation itself. In most situations one cannot expect transfer to occur unless there has been some improvement exhibited in the trainer itself. Such results are therefore useful, though they cannot actually replace transfer measures stated in terms of the criterion task.

Training effectiveness cannot be measured by means of correlation coefficients. The concepts of reliability and validity of the measures obtained on the device therefore have no place in studies whose purpose it is to evaluate the effectiveness of a device for performance improvement. After all, the traditional psychometric approach deliberately tries to minimize practice effects, whereas in training they must be emphasized. A correlation coefficient indicating a high validity relationship between a device and an operational situation means simply that the same abilities are involved in both tasks. It does not show that practice on the first task can lead to improvement on the second; in fact, it tells nothing about this relationship. Obviously, too, practice on a device may bring about improvement whether behavior is measured or not. This implies, incidentally, that a scoring mechanism is not a necessity when the device is used for training, though it may be desirable for other reasons.

In summary, the training effectiveness of a device is determined in terms of the measurement of transfer from training on the device to performance in an operational situation. Research in this area is often concerned with more detailed questions such as the effects of changes in length, arrangement, and sequence of practice within a training course and their relations to amount of transfer.

Coefficients of correlation measure the extent to which common abilities are involved in the device and operational tasks but do not provide measures of training improvement.

CONCLUDING REMARKS

This paper has attempted to describe and clarify some research issues which occur in connection with the development, use, and evaluation of training devices. The purpose has been to see whether such clarification can reveal a framework for psychological research in the training devices area.

It is maintained that the kinds of utilization of training devices are two: performance measurement and performance improvement. Although the two uses are frequently made of a single piece of equipment, they may be distinguished particularly in the characteristics of the device which are essential for each purpose. When the device is used for performance measurement, the important characteristics are reliability and validity. When the device is employed for improving performance, on the other hand, the characteristic of importance is the amount of transfer of learning to an operational task. In either case, degree of simulation becomes a secondary consideration.

When one considers these purposes, some of the opportunities for research on training devices become apparent. On the side of methodology, the range of these problems includes job analysis, training, proficiency measurement, and criterion development. On the side of theory, questions mentioned in the present discussion include those of the structure of skills, the determinants of human variability, relationships of set and motivation to learning, and the mechanisms of transfer of learning.

REFERENCES

1. ADKINS, DOROTHY C. Construction and analysis of achievement tests. Washington, D. C.: U. S. Government Printing Office, 1947.
2. BATSON, W. H. Acquisition of skill. Psychol. Monogr., 1916, 21, No. 2 (Whole No. 91).
3. BILODEAU, E. A. Acquisition of skill on the Rudder Control Test with various forms of social competition. USAF Hum. Resourc. Res. Cent., Res. Note P&MS, 51–6, 1951.
4. BILODEAU, E. A. A preliminary study of the effects of reporting goals as a function of different degrees of response accuracy. USAF Hum. Resourc. Res. Cent., Res. Bull., 52–4, 1952.
5. BILODEAU, E. A. A further study of the effects of target size and goal attainment upon the development

of response accuracy. *USAF Hum. Resourc. Res. Cent., Res. Bull.,* 52–7, 1952.

6. BILODEAU, E. A. Some effects of various degrees of supplemental information given at two levels of practice upon the acquisition of a complex motor skill. *USAF Hum. Resourc. Res. Cent., Res. Bull.,* 52–15, 1952.

7. BILODEAU, E. A., & MORIN, R. E. Proficiency on the Pedestal Sight Manipulation Test with and without the tracking pipper. *USAF Hum. Resourc. Res. Cent., Res. Bull.,* 51–27, 1951.

8. BRIDGMAN, C. S., GRAY, FLORENCE, & SOLOMON, R. L. A graphical analysis of sighting performance on B-29 sights. OSRD Report No. 5221. Applied Psychology Panel, NDRC, Project AC–94, June 1945.

9. BROGDEN, W. J., & SMITH, K. U. Summary of research and development on project AC–94: psychological factors in the operation of flexible gunnery equipment. OSRD Report No. 6212. Applied Psychology Panel, NDRC, Project AC–94, October 1945.

10. BROWN, J. S., & JENKINS, W. O. An analysis of human motor abilities related to the design of equipment and a suggested program of research. In P. M. Fitts (Ed.), Psychological research on equipment design, *AAF Aviation Psychology Program Report No. 19.* Washington, D. C.: U. S. Government Printing Office, 1947. Pp. 35–63.

11. CARR, H. A. Teaching and learning. *J. genet. Psychol.,* 1930, **37**, 189–218.

12. CRAIG, D. R., & ELLSON, D. G. The design of controls. In *National Research Council, Human factors in undersea warfare.* Washington, D. C.: Committee on Undersea Warfare, National Research Council, 1949. Pp. 133–151.

13. EDGERTON, H. A., & FRYER, D. H. The development of an evaluation procedure of training aids and devices. Port Washington, L. I., N. Y.: U. S. Navy Special Devices Center, 1950. (Tech. Rep., SDC 383–2–1.)

14. GAGNÉ, R. M. Learning and transfer of training in two forms of rudder control task. *USAF Hum. Resourc. Res. Cent., Res. Note P&MS,* 50–1, 1950.

15. GAGNÉ, R. M., FOSTER, HARRIET, & CROWLEY, MIRIAM E. The measurement of transfer of training. *Psychol. Bull.,* 1948, **45**, 97–130.

16. GOLDSTEIN, M., RITTENHOUSE, C. H., & WOODS, J. P. Studies of performance on the E-26 Flexible Gunnery Trainer. *USAF Hum. Resourc. Res. Cent., Res. Bull.,* 52–17, 1952.

17. GRANT, D. A., & SCHIPPER, L. M. The acquisition and extinction of conditioned eyelid responses as a function of the percentage of fixed-ratio random reinforcement. *J. exp. Psychol.,* 1952, **43**, 313–320.

18. GUTHRIE, E. R. *The psychology of learning.* New York: Harper, 1935.

19. HARLOW, H. The formation of learning sets. *Psychol. Rev.,* 1949, **56**, 51–65.

20. HOUSTON, R. C. The function of knowledge of results in learning a complex motor skill. Unpublished master's thesis, Northwestern Univer., 1947.

21. HUNTER, W. S. Learning: IV. Experimental studies of learning. In C. Murchison (Ed.), *Handbook of general experimental psychology.* Worcester: Clark Univer. Press, 1934. Pp. 497–570.

22. KAO, DJI-LIH. Plateaus and the curve of learning in motor skill. *Psychol. Monogr.,* 1937, **49**, No. 3 (Whole No. 219).

23. LINDAHL, L. G. Movement analysis as an industrial training method. *J. appl. Psychol.,* 1945, **29**, 420–436.

24. MAHLER, W. R., & BENNETT, G. K. Psychological studies of advanced Naval Air Training: evaluation of operational flight trainers. Port Washington, L. I., N. Y.: U. S. Navy Special Devices Center, 1950. (Tech. Rep., SDC 999–1–1.)

25. MORIN, R. E., & GAGNÉ, R. M. Pedestal Sight Manipulation Test performance as influenced by variations in type and amount of psychological feedback. *USAF Hum. Resourc. Res. Cent., Res. Note P&MS,* 51–7, 1951.

26. MURPHY, G., MURPHY, LOIS B., & NEWCOMB, T. M. *Experimental social psychology.* (Rev. Ed.) New York: Harper, 1937.

27. SEASHORE, H. G., KURTZ, A. K., KENDLER, H., STUNTZ, S. E., & RAPPAPORT, C. Variation of activities in code classes: an experimental study of the problem of monotony in code learning. OSRD Report 4082. New York: The Psychological Corporation, 1944.

28. SKINNER, B. F. Some contributions of an experimental analysis of behavior to psychology as a whole. *Amer. Psychologist,* 1953, **8**, 69–78.

29. THORNDIKE, R. L. *Personnel selection.* New York: Wiley, 1949.

30. WILLIAMS, A. C., JR., & FLEXMAN, R. E. An evaluation of the Link SNJ operational trainer as an aid in contact flight training. Port Washington, L. I., N. Y.: U. S. Navy Special Devices Center, 1949. (Tech. Rep., SDC 71–16–3.)

31. WOLFLE, D. The use and design of synthetic trainers for military training. OSRD Report 5246. Washington, D. C., 1945.

32. WOLFLE, D. Training. In S. S. Stevens (Ed.), *Handbook of experimental psychology.* New York: Wiley, 1951. Pp. 1267–1286.

THE USE OF HUMAN SUBJECTS IN PSYCHOLOGICAL RESEARCH[1]

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FOR some years the APA has had a Committee on Precautions in Animal Experimentation and has published rules regulating the use of animals for research purposes. This Committee and the animal experimentation code were established largely as an answer to the wild accusations about "dog torture chambers" hurled by antivivisectionists. The code provided a clear public statement of how animal experimentation was carried out, and the Committee could investigate charges that any of the code's provisions had been violated.

In the past no particular attention appears to have been paid to the use of human subjects in psychological research, apparently because no general problem existed as it did for animal experimentation. Of course, one heard gossip at times about a local uproar in some college community when students were asked to complete questionnaires which dealt with sex, race relations, or other emotionally charged material. Similar protests occasionally followed stress or frustration experiments or somatotype studies in which nude or nearly nude subjects were photographed. But such public outcries were rather rare and chiefly of local concern. Certainly they could in no way be compared to the nationwide clamor about animal research instigated by the antivivisectionists. Indeed, it was not until 1953 that a formal statement was issued by the APA concerning the general welfare of human research subjects. This may be found in *Ethical Standards of Psychologists*, published by the APA.

Like psychology, the medical sciences had no noteworthy problem in the use of human subjects for research purposes. The physician's responsibilities for his individual patient were carefully defined, and it was presumed that a subject in any medical experiment would be treated as if he were a patient. But on the heels of the Allied victory in

Europe in World War II came stories of incredibly barbarous medical experiments performed on human subjects by Nazi physicians in the name of science. When the documentation of these atrocities was published (6), the integrity of the entire profession of medicine was challenged; for the German doctors involved were no mere tools or quasi-charlatans. They were leaders in their profession, and they themselves planned and executed their program of deliberate maiming and lingering death. It was out of these findings of the Nuremberg trials that the present concern over the use of human subjects in research grew. While writings on the problem have come chiefly from medical persons, all sciences which use human subjects for research are to some degree involved. As Shimkin put it, "Responsibilities do exist, and it is better to define them and see that they are not abused than to deny their existence and accept the consequences of denial" (7, p. 205).

The psychologist who is engaged in research with human subjects is not likely to get into trouble if he adheres firmly to the principles of *consent, confidence,* and *standard* or *acceptable procedure.* Since it is highly improbable that psychologists direct research which may imperil the lives of subjects, a risk which is present in some medical experimentation, it is believed that these principles will suffice for virtually all research in which psychologists utilize the services of human subjects. However, because psychologists occasionally design research for which a physician assumes responsibility, the basic principles governing medical experiments with human subjects, as set forth by Taylor (8, p. xxiii) after the Nuremberg trials, will be presented here later.

When psychological research has aroused public ire, it has probably occurred most often when the principle of *consent* was violated in connection with some cherished cultural value. The uproars over questionnaires which students found personally obnoxious or the protests against being photographed

in the nude are cases in point. Had the subjects been permitted to volunteer for the investigations, it is unlikely that such difficulties would have arisen. Many items of personal information are so trivial that it may appear unnecessary to invite subjects clearly and pointedly to participate, and to permit any to withdraw who wish to do so. Yet what is trivial to one is a threat to another. An innocent question on annual income smacks of internal revenue checkups to some persons, and a question on attitude is reminiscent of the Gestapo to others. The only safe procedure is to invite participation while avoiding any appearance of coercion and to make it easy for any subject to withdraw gracefully. Where the information requested is highly personal or where the experiment involves some pain, discomfort, or risk, the subject should be made fully aware of what he is consenting to, at least in a general way. If the subject is to be whirled in a Barany chair, electrically shocked, or quizzed about his sex practices, prudence dictates that the subject should be told what he is in for. Kidd (5, p. 212) remarked that ". . . the law does not regard trifles" —de minimis non curat lex; the public, however, often does. Further, the trifle occasionally has serious consequence. The present writer once saw a subject seized by a convulsion after ice water was squirted in his ear to demonstrate nystagmus. The subject remarked afterward that he never would have served had he known what was going to be done to him.

Upon occasion the consent of the subject himself cannot be obtained because he is legally incapable of giving it, as in the case of mental patients. In such instances the family or the physician or other person responsible for the patient's welfare may grant the necessary permission. Another special occasion concerns the use of case history or psychological test data without obtaining prior consent of the persons involved. Often such records are old and the subjects too numerous or too scattered geographically to make such consent feasible. Yet college and other institutional officials sporadically raise this question. If the persons concerned are not harmed by the use of their records and their identities are not publicly revealed, there is no problem and their consent for the professional use of such existing records is not necessary.

The second principle, that of confidence, relates to more than the confidential nature of the personal information concerning the subjects used in research. It also relates to public confidence in psychology itself. The activities of the research and the practicing psychologist converge closely at this point. If psychology is to be regarded as a mature profession, more than skill and knowledge must be demonstrated. The trust in which the psychologist holds personal information must be taken for granted by the public. This is not merely a legal control in the sense of avoiding libel or slander suits. Indeed, the psychologist could probably disclose any information about his subjects or clients in the performance of a public, legal, or moral duty without fear of legal penalty, provided he did so without malice. This seems to be a generally accepted principle of the law. Thus, the problem is not one of legal immunity, but rather one of guarding personal data concerning a client, patient, or subject in an experiment so closely that such knowledge in the psychologist's possession will be publicly regarded as privileged. A number of states have no statute granting privileged communication to physicians; yet because the public assumes that such privilege exists, no problem is encountered. The State of Illinois, for example, has no such statute; but as Guttmacher and Weihofen (3, p. 270) note, the courts have upheld the psychiatrist when he has refused to testify in regard to a patient.

Just what should be regarded as confidential is something of a problem. About the only statement that can be made is that good sense and good taste must prevail. Should occasion arise to disclose the fact, whether a subject is 5 or 6 feet tall is obviously not a matter for secrecy but whether he is afflicted by fetishistic behavior is. A subject's psychological test scores, for example, are properly kept confidential, despite the fact that such data are often handled carelessly. A number of psychological clinics and counseling bureaus, as cases in point, will not transmit any information about a client or patient without his written consent. This procedure should be standard for agencies which disclose such information to persons outside of their own professional organization.

It is significant that Kinsey's sexual behavior studies have not encountered opposition in the areas here under discussion. Kinsey uses only subjects who volunteer and who know what they are in for, and he guarantees confidence. The public reference to coded material, locked strongroom files, etc. enable Kinsey to obtain histories which would

otherwise be unavailable to him. The parallel one may draw is that when any information is given in confidence, the welfare and advancement of our science demands that the identity of the subject be completely protected. If material is not given in confidence and if good taste and good sense do not clearly indicate that the information should be treated as confidential, the only rule to follow in such doubtful cases may be put tersely: *hold your tongue and pen.*

The third principle governing the use of human subjects in psychological research is that of *standard* or *acceptable procedure*. This assumes that the experimenter is competent in the area of his research undertaking and the procedure is standard in the sense of having been tried many times previously by many investigators. If the procedure is novel, and many procedures must be novel if we are to advance, the procedure must be regarded as acceptable by competent psychologists. That is, psychologists of established reputation in the field should have examined the proposed new procedure as well as any attendant risks and approved the approach as acceptable. Adherence to this principle of standard or acceptable procedure is a legal safeguard. No one has been successfully sued, so far as the present writer is aware, on grounds that a memory drum drove a subject psychotic or that an electric shock caused mental or bodily harm in a psychological experiment. Nor is there likely to be a successful suit so long as it can be demonstrated that the procedure has been widely used without detriment to other subjects and that obvious precautions were taken, such as using properly insulated wires, having a physician available where physical danger exists, etc. But it should be noted in this connection that some lawyers who specialize in industrial accident claims are quietly reviewing the problem of lawsuits based upon the charge that neurotic or psychotic behavior was produced by the stresses of a particular job. A word to the wise should suffice.

The importance of this third principle goes beyond legal protection in that, like the principles of consent and confidence, the esteem in which the psychological profession is held is directly involved. At least one therapist who personally used sexual intercourse as an experimental treatment for some of his female patients is now languishing in prison. Despite the fact that his procedure could in no sense be regarded as standard or acceptable, the

public reaction was one of outrage and psychology suffered a severe blow. Another research study, though it was never actually carried out, is of interest in this reference frame. A counseling psychologist spent considerable time designing a study in which groups of clients would be given varying interpretations of their test results. Some would be given accurate interpretations while others who scored low would be told they scored high and others who scored high would be told they scored low, etc. The study was ingeniously designed and encompassed several critical points by carefully controlling the effects of counseling, motivation, and the like. But however important the study might be, the possible effects upon the subjects are horrible to contemplate. Only a psychopath could execute such research. Furthermore, much the same information could be gathered by controlled studies of counseled versus noncounseled subjects and, most important, without risk to the subject.

In some respects military personnel are in a special category with respect to service as experimental subjects and submission to medical (and presumably psychological) treatment. Of course, any military person has the same right to volunteer or not to volunteer as a civilian subject in an experiment. However, it appears to be essential that such service should not hamper or incapacitate the subject in the performance of his military duties. There is, however, an old tradition of human experimentation in the military services, the most famous example of which is the yellow fever studies of Walter Reed. Currently, the authorized fields of research in the military forces include virtually every field of psychology. The psychologist who is engaged in research with military personnel is urged to read the article by W. H. Johnson (4) of the Judge Advocate Corps, United States Army.

In passing judgment upon the Nazi physicians who were charged with atrocities disguised as medical research, the Nuremberg tribunal laid down 10 basic principles governing permissible medical experiments. These principles are quoted by General Telford Taylor in Mitscherlich and Mielke's (6, p. xxiii) *Doctors of Infamy*. These principles are regarded as so basic and so important to psychology that they are herewith quoted in full:

1. The voluntary consent of the human subject is absolutely essential. This means that the person involved should have legal capacity to consent; should be so situ-

ated as to be able to exercise free power of choice, without the intervention of any element of force, fraud, deceit, duress, overreaching, or other ulterior form of constraint or coercion; and should have sufficient knowledge and comprehension of the elements of the subject matter involved as to enable him to make an understanding and enlightened decision. This latter element requires that before the acceptance of an affirmative decision by the experimental subject there should be made known to him the nature, duration, and purpose of the experiment; the method and means by which it is to be conducted; all inconveniences and hazards reasonably to be expected; and the effects upon his health or person which may possibly come from his participation in the experiment.

The duty and responsibility for ascertaining the quality of the consent rests upon each individual who initiates, directs, or engages in the experiment. It is a personal duty and responsibility which may not be delegated to another with impunity.

2. The experiment should be such as to yield fruitful results for the good of society, unprocurable by other methods or means of study, and not random and unnecessary in nature.

3. The experiment should be so designed and based on the results of animal experimentation and a knowledge of the natural history of the disease or other problem under study that the anticipated results will justify the performance of the experiment.

4. The experiment should be so conducted as to avoid all unnecessary physical and mental suffering and injury.

5. No experiment should be conducted where there is an a priori reason to believe that death or disabling injury will occur; except, perhaps in those experiments where the experimental physicians also serve as subjects.

6. The degree of risk to be taken should never exceed that determined by the humanitarian importance of the problem to be solved by the experiment.

7. Proper preparations should be made and adequate facilities provided to protect the experimental subject against even remote possibilities of injury, disability, or death.

8. The experiment should be conducted only by scientifically qualified persons. The highest degree of skill and care should be required through all stages of the experiment of those who conduct or engage in the experiment.

9. During the course of the experiment the human subject should be at liberty to bring the experiment to an end if he has reached the physical or mental state where continuation of the experiment seems to him to be impossible.

10. During the course of the experiment the scientist in charge must be prepared to terminate the experiment at any stage, if he has probable cause to believe, in the exercise of the good faith, superior skill, and careful judgment required of him that a continuation of the experiment is likely to result in injury, disability, or death to the experimental subject.

REFERENCES

1. ANONYMOUS. Regulation of psychological counseling and therapy. *Columbia Law Rev.*, 1951, **51**, 474–495.
2. GUTTENTAG, O. E. The physician's point of view. *Science*, 1953, **117**, 207–210.
3. GUTTMACHER, M. S., & WEIHOFEN, H. *Psychiatry and the law.* New York: Norton, 1952.
4. JOHNSON, W. H. Civil rights of military personnel regarding medical care and experimental procedures. *Science*, 1953, **117**, 212–215.
5. KIDD, A. M. Limits of the right of a person to consent to experimentation on himself. *Science*, 1953, **117**, 211–212.
6. MITSCHERLICH, A., & MIELKE, F. *Doctors of infamy.* New York: Schuman, 1949.
7. SHIMKIN, M. B. The problem of experimentation on human beings. I. The research worker's point of view. *Science*, 1953, **117**, 205–207.
8. TAYLOR, T. Statement pp. xvii–xxvi in Mitscherlich, A., and Mielke, F. *Doctors of infamy.* New York: Schuman, 1949.

SEMINAR ON "PROFESSIONAL ASPECTS OF PSY-CHOLOGICAL SCIENCE" AT THE UNIVERSITY OF MARYLAND

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OUR department has for several years been conducting a staff-directed seminar on "Professional Aspects of Psychological Science," as an early course in the core curriculum for beginning graduate students, as a required course for transfer graduate students, and as an elective course for selected senior majors. Daniel (1) has presented an outline of a somewhat similar course at the University of Missouri, and the two courses may be compared. A recent book by Daniel and Louttit (2) entitled *Professional Problems in Psychology* will no doubt interest other graduate departments in such a course.

We believe that the central emphasis we have placed on the seminar in our course sequence and our experience with the seminar might prove of interest and value to other departments including or considering such a seminar in their graduate programs. We have therefore prepared this short note to describe our purposes, the content and emphasis of the seminar, the procedure followed, and to present some results of our experience along with certain basic unsolved problems which still remain.

The seminar differs in its objectives from all our other courses and seminars. It is concerned with the integration of those problems reflected in the community of professional psychologists. We try to acquaint the students with the major problems which psychology is facing in its development as a profession. The seminar also provides an informal opportunity for students to meet and discuss significant problems with all members of the staff. For the beginning graduate student and the invited senior this is a first experience with a real seminar, so it also serves to acquaint them with the characteristic procedures of such a course. By providing realistic information on professional functions and job requirements, we are attempting to help our students decide on fields of specialization, and vocational choice and opportunity. We inform them of the nature, characteristics, and history of the professional organizations and the relations of psychologists to other professions, endeavoring to impress them with the responsibilities of professional conduct. Finally, we are trying in a systematic manner to acquaint them with the programs for the training of psychologists, the nature of the communication links among psychologists, etc.

This two-hour seminar meets once a week for one semester and is given for two credits, in contrast with our regular three-credit courses. The only grading used is satisfactory or unsatisfactory performance. All members of the staff take part in the planning of the seminar, and several staff members attend each meeting. Reading lists, which students use in preparing themselves for the discussion of the topics shown in the course outline, are furnished for each meeting. In addition to staff contributions, the executive secretaries of the APA, Dael Wolfle and F. H. Sanford, have appeared as guest speakers on the central theme of the professional problems of psychology.

The seminar is organized around four major topics. An introductory session is held where all the participants, students and staff, are present; and the seminar is closed by a final evaluation session.

We have given the seminar three times, and each time major modifications have been made. A general outline of the topics is shown below.

I. Introduction and Orientation
A. Purposes of the seminar
B. Techniques to be employed
C. Problems to be explored

II. The Professional Psychologists
A. Organizations: the constitution and objectives of APA and its divisions and committees, state societies, interest groups, and foreign and international organizations and their functions
B. Research programs and agencies: governmental—civilian and military; funding organizations; private profit and non-profit agencies.
C. Job classifications and opportunities

1. Academic fields

a. Employment opportunities and professional pursuits; salaries; conditions and requirements of employment; tenure; etc.

2. Industrial, polling, and market research areas

a. Employment opportunities and professional pursuits; salaries; conditions and requirements of employment; tenure; etc.

3. Governmental and military agencies

a. Employment opportunities and professional pursuits; salaries; conditions and requirements of employment; tenure; etc.

4. Clinical, guidance, and public service fields

a. Employment opportunities and professional pursuits; salaries; conditions and requirements of employment; tenure; etc.

III. Problems and Standards of Professional Psychologists

A. Professional responsibilities

B. Ethics and codes

C. Licensing, certification, and control of malpractice

D. Relations with other professions, including medicine and psychiatry

IV. Professional Training

A. The major graduate departments: their histories, characteristics, emphases, faculty, etc.

B. The literature: APA and non-APA journals; foreign literature; nonpsychological literature; history, support and publication, editorial policies of journals, etc.

C. Techniques of literature searches: Abstract sources—psychological, biological, medical, etc.

D. Report preparation: requirements, format, techniques, etc.

V. Trends in the development of professional problems

A. Discussion by guest speaker

B. Group evaluation

VI. Evaluation of Seminar

A. Student

B. Staff

Now that we have indicated what we are trying to do and how we do it, we should like to present the results of experience, together with some questions.

It is extremely difficult to make cogent generalizations regarding our experience with the seminar. We use student and staff evaluations as a basis for changes each year. Students have reported that they have secured information valuable to them during the seminar. At times there remained undiscussed and unanswered problems. In our planning we tried to start with those questions about psychology as a profession in which our students were interested. We also had some ideas on what they should be dealing with in the seminar. Sometimes these student questions and our ideas fitted. Sometimes they did not.

Little general enthusiasm was generated for examination of the programs of other departments or for the examination of the means of communication in psychology. The participants seemed more interested in the derivatives of specific employment: salaries, freedom of operation, nature of duties, etc. Students had a real interest in ethical problems, perhaps out of proportion in intensity with the reality of such considerations in their future performance as professional psychologists. Hackman (3) has already described this interest in ethics.

In general we feel that some useful things have been accomplished by our efforts and by the work of our students. There remain many unanswered questions, a few of which we shall pose, together with our current feelings about them.

1. Is it necessary or desirable to include such a seminar, formal or informal, in the graduate program? We might argue that many of us did not have such a seminar and are no worse off. It can be said that students will eventually perceive the problems and learn the appropriate behaviors. We believe, however, that there is some value to the student in an introduction to professional problems and an acquaintance with professional responsibilities early in graduate training.

2. Would this seminar be more profitable later in the graduate program than in the first year? Is the challenge of learning something of the content of our science so great that the professional problems have little real import? We believe that early exposure does no harm, and may interest students in problems they have not considered.

3. Should we reinforce the social service motive of the beginning graduate student by an emphasis on the ethics of our profession? Or do we learn these problems slowly, with difficulty, with errors, by facing real problems and being forced to make decisions? We believe that an intelligent presentation of major problems divorced from the emotion-laden decision-making situation may have value.

4. Can our students just as readily learn about our journals, departments, training programs, and employment opportunities from their further graduate studies or development as psychologists? We believe that they probably can. Maybe our seminar will make it easier.

5. Are students who have taken part in such a seminar better prepared for their work as psychologists when compared to those who have just "grown up?" We don't know, but would lean to an affirmative answer.

We are still developing this seminar, because we feel that the purposes of the seminar are important, and several values accrue from it. In this development we cordially invite comments and criticisms from other psychological groups that take an interest in this form of training.

REFERENCES

1. DANIEL, R. S. Seminar on professional problems of psychology. *Amer. Psychologist*, 1950, 5, 161.
2. DANIEL, R. S., & LOUTTIT, C. M. *Professional problems in psychology*. New York: Prentice-Hall, 1953.
3. HACKMAN, R. C. Comments on the training of graduate students in ethics in psychology. *Amer. Psychologist*, 1952, 7, 429–430.

DOCTORAL DISSERTATIONS IN CLINICAL TRAINING PROGRAMS: 1948–1953

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ONE of the major contributing factors to the growth of the American Psychological Association in general is the recent phenomenal expansion of the field of clinical psychology. The recognition of clinical psychologists during and following the war brought about a great upsurge in training and training facilities, especially at the doctoral level. The extent of this training trend and the research trends revealed in the dissertations completed in these doctoral training programs are the main concern of the present report.

Early in 1953 a letter was sent out, by the author, to the directors of all the APA "approved" clinical training programs—40 in number.[1] In the letter a request was made to list all authors and titles of dissertations completed by the trainees of the clinical psychology training programs since 1948. One additional letter was sent out to an approved school which no longer has a clinical training program. Thus, in all, 41 requests for information were issued. Complete returns were obtained by the end of May. Consequently, the data to be presented are based on 100 per cent returns from the university training programs that were polled.

Growth Trend

A total of 781 dissertations completed by students in clinical psychology training programs for the period 1948–1953 was reported. It must be pointed out that the information for 1953, for obvious reasons, is far from complete. Most of

[1] This survey was carried out by the author as chairman of the Committee on Teaching Clinical Psychology, Division of Clinical and Abnormal Psychology.

the dissertation titles reported for this year were completed at the end of the Winter Quarter, while a few others were completed, but the degree was not to be granted until the end of the spring.

Table 1 gives the distribution of dissertations completed in the schools represented in the survey. It is particularly interesting to note that more than 75 per cent of the dissertations were completed in 19 (less than 50 per cent) of the larger schools. Nearly one-third of the smaller schools produced less than 10 per cent of the dissertations. On the other end of the scale—only three schools (Michigan, Pittsburgh, and T. C. Columbia) produced more than 16 per cent of the total number of dissertations. An examination of the raw data indicates, however, that not only the size of the school, but also the date of the beginning of the training program in clinical psychology, is a factor in the productivity of doctoral research.

Table 2 shows the constant expansion of the field of clinical psychology via degrees granted (dis-

TABLE 1

DISTRIBUTION OF DOCTORAL DISSERTATIONS
IN THE TRAINING SCHOOLS

Number of Studies	Schools	Studies	Percentage of Total
1–9	13	70	9.0
10–19	9	121	15.5
20–29	9	227	29.0
30–39	7	237	30.2
40–49	3	126	16.2
Total	41	781	99.9

TABLE 2

DOCTORAL RESEARCH PRODUCTIVITY: 1948–1953

Year	Number of Schools	Studies	Percentage of Total
1948	18	34	4.4
1949	21	77	9.9
1950	26	155	19.8
1951	35	189	24.2
1952	39	253	32.4
(1953)*	(23)	(73)	9.3

* Data for 1953 are only through the month of April.

sertations completed) since 1948. Of course, the data for 1953 are incomplete, and, consequently, it is impossible to state that the growth trend is still continuing. Not only has the number of dissertations been increasing steadily from year to year, but also the number of schools in which they were completed has been growing rapidly. The number has more than doubled during the five-year period. It is quite obvious that doctoral research in clinical psychology is constantly increasing and emanating from an increasingly larger number of universities with approved training programs.

Research Trends

Titles of dissertations differ widely with respect to the amount of information they offer regarding the investigations undertaken and completed. Some are vague and extremely general, while others are detailed and actually are descriptive of the subject matter covered in the dissertations. Despite the differences in amount of information gained from the titles, it was felt that a rough classification into broad areas of research may be possible. This was actually attempted and the numerical findings, in terms of percentages of the total number of dis-

sertations completed in a particular year, are presented in Table 3.

A word of explanation of the column headings in the table is in order. The first column headed by the title "general, experimental, theoretical" includes all studies completed by trainees of clinical programs which are strictly in the "general psychology" tradition. These are studies which are concerned with psychological variables that are remotely (if at all) related to the *practice* of clinical psychology. A few of these experiments are with animals.

The second column gives the percentages of dissertations that deal with significant personality variables, examination of theoretical concepts in the area of personality dynamics, as well as general social processes and issues concerned with interpersonal relationships. A few cultural-anthropological dissertations were included in the classification.

Columns 3, 4, 5, and 6 represent the more strictly "relevant" *clinical* studies. The third category (column 3) is characterized by two major features. In the first place, the major focus in these studies was some deviant clinical population (mainly schizophrenia and the neuroses); secondly, they are experimental approaches to the testing of some specific variables related to the theoretical orientations with respect to these disorders.

The studies in the fourth category also deal with special populations, but utilize in their designs various accepted clinical instruments in the hope of discovering with their aid some characteristics which may be meaningful and diagnostic of the conventional psychiatric groupings. Thus, these studies attempt to uncover some lawfulness in the responses of comparatively ill-defined groups to

TABLE 3

PERCENTAGES OF DISSERTATIONS IN SOME MAJOR CATEGORIES

Year	General, Experimental, Theoretical (1)	Personality and Social Processes (2)	Experiments with Deviant Groups (3)	Test Responses and Patterns in Special Populations (4)	Experimentations with Diagnostic Techniques (5)	Methods of Psychological Treatments (6)	Physiological Processes and Physiological Therapies (7)	Miscellaneous Topics
1948	3	21	3	21	21	15	6	10
1949	5	22	21	14	17	8	1	12
1950	11	19	12	12	21	12	6	7
1951	12	17	18	6	23	12	5	6
1952	14	21	22	9	15	6	4	9
(1953)*	19	19	19	4	20	1	6	12
1948–53	11.9	19.6	18.3	9.3	19.0	9.0	4.7	8.2

* Data incomplete—only through April, 1953.

predominantly unstructured (projective) material, the stimulus value of which is not fully understood.

The studies in the fifth category, however, are not concerned with special populations, but are concerned with the instruments or diagnostic devices used by the clinician; their validity, reliability, and effectiveness under varying conditions; and with the measurement of specific personality variables in nondeviant populations.

The sixth category includes dissertations dealing with the psychotherapeutic process, primarily. A few of the studies deal with investigation of modification of behavior with psychological means such as hypnosis, not strictly classifiable as psychotherapy in the usual sense.

In the seventh category, small percentages are listed, consisting primarily of physiological investigations and studies of effects of physical therapies such as electric shock, insulin, etc. upon groups of subjects.

Finally, in the last column, the percentages of miscellaneous studies are summarized. Actually a number of other categories could have been formed, but the number of the studies in each was too small. Besides, the over-all classification and tabulation would have become too cumbersome and unwieldy. It will suffice to state that dissertations on occupational groupings and problems, genetic aspects, and parent-child relationships, as well as technical psychometric issues were the major groups constituting the "miscellaneous" category.

A few outstanding trends and directions in the doctoral research in clinical training programs may be gleaned from the data presented in Table 3.

1. There seems to be a constant increase in the absolute and relative numbers of general-experimental dissertations authored by graduates of clinical training programs. Whereas very few clinical students produced theses in these areas in 1948 and 1949, the number in 1952 rose to 36, or 14 per cent of the total dissertations for the year. The 1953 trend is in the same direction.

2. With some minor fluctuations, the interest in basic personality variables and social processes remains comparatively constant during the five-year period covered by this survey. A similar constancy, after a humble beginning, is reached in the area of experimentation with clinical groups. The major trend here is to establish well-controlled experimental conditions and see if measurement of certain personality variables and reaction patterns is relevant to the a priori psychiatric differentiation.

3. Most interesting are the categories of dissertations in which the use of clinical instruments is of major importance. There is, quite obviously, a decline in research with test responses on special populations. There is no longer a significant tendency to give some test, projective or objective, to a group of psychiatric patients and normals and hope that some "pattern," "cluster," or "sign" may be elicited that will be of "diagnostic significance." Instead, there is a more dominant and constant trend to be more critical of the clinical tools in current use. Attempts to vary conditions of testing, stimulus materials, conditions of subjects, etc. fall in this category (category 5). Problems of reliability, validity, and test findings in relation to specific personality variables in nonclinical populations are increasingly the concern of doctoral research. The focus has shifted from test findings in a specific diagnostic category to the psychological meaning of the time-honored tests themselves and to the specific personality characteristics they may represent.

4. Lamentable is the comparative paucity of research in the area of psychotherapy (category 6). The number of dissertations in this area in any one year did not exceed 23 (12 per cent of the 1951 total). However, the time required for the completion of studies in this area and the type of data necessary will probably continue to make psychotherapeutic investigations a relatively infrequent undertaking for doctoral research.

5. If we are to define clinical research in a narrow sense, i.e., research with clinical tools and/or clinical populations, then about 60 per cent of all the dissertations fall into this classification (categories 3, 4, 5, 6, and some of 8). The trend is fairly constant over the years covered by this survey. Forty per cent of the dissertations are not so close to the clinical psychologist's "base of operations." They represent the types of investigation that might well have been executed by his colleagues in the fields of experimental, social, theoretical, physiological, and personality psychology. If doctoral research is to be taken as a criterion, therefore, one need not be alarmed by the alleged "cleavage" in the field of psychology. Generally speaking, doctoral research in clinical psychology is research on significant variables in human psychology with increasing methodological sophistication and with problems of social consequence.

THE APA STUDY OF PSYCHOLOGISTS

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THE American Psychological Association received, in October of 1952, a grant from the National Science Foundation for the purpose of surveying the current status of psychological science. This grant was made in response to a proposal from the Policy and Planning Board of the APA [1] for support of two projects: (a) an Evaluation of Psychological Science, which is known as Project A, and (b) the Occupational, Educational, and Institutional Relations of Psychology, which is known as Project B.

The programs of both of these projects have been developed in planning sessions of the Advisory Committee on the Development and Status of Research and Education in Psychology (a Policy and Planning Board committee), of which Dael Wolfle is chairman. The more detailed plans for Project B have been developed by a committee with Raymond A. Bauer, M. Brewster Smith, John Stalnaker, Robert L. Thorndike, and the writer as members. Two full-time staff members are currently employed to assist in work on the latter project. These are Norma Metzner, located in the APA office in Washington, and Genevieve O. Rogge, who is conducting the Providence survey described later.

Since the design of Project B requires the cooperation, understanding, and support of many members of the APA, it has seemed desirable to report the nature of plans at this time, even though some of the planning and quite a bit of the data collection and analysis remain to be done. Project B conceives as its major purpose the collection of data which may lead to (a) the better understanding of personal and environmental factors that influence the research productivity or the professional contribution of the individual psychologists, (b) increased appreciation of society's demands for and use of psychological services, and (c) an understanding of the influence of various programs of education and training in psychology. Several interrelated lines of attack are being followed in the work of this project; these are described in succeeding paragraphs.

[1] See the Annual Report of the Policy and Planning Board, *Amer. Psychologist*, 1952, 7, 563–568.

A Study of Research Psychologists

An examination of the research literature in any area of psychology cannot fail to impress the reader with the obvious fact that a large portion of current progress in psychology results from the labors of a few. One might conjecture about the effects of the addition of a small increment of equally productive persons on the total rate of progress in psychology as a science, and might well ask how such an increment might be obtained. Some suggestions for programs to attain this increment might emerge if we understood how the psychologist's background, his training, his job history, the nature of his current employment, and his attitudes and values influence his research productivity.

This study attempts to provide such types of information by comparing a sample of psychologists considered to have made a significant contribution in research with a sample of their contemporaries. Since the emphasis is placed on research productivity, this sample is limited to psychologists who have received a PhD or an equivalent degree. Psychologists being studied are those who received a doctor's degree during the interval 1930 to 1944, inclusive, and who are members of the APA. (Names of persons meeting this requirement except for membership in the APA have been obtained, and represent an additional group to be studied.) Analyses are made separately for persons receiving their degrees in each of the three five-year periods, 1930–1934, 1935–1939, and 1940–1944.

Among the characteristics to be studied are such matters as social origins, undergraduate college records, nature of the setting of graduate training, circumstances under which the dissertation was done, nature of the first postdoctoral employment obtained, current status, current attitudes toward research, interests and values with regard to research activities and to research contributions and contributors, and current distribution of time among such activities as research, service, administration, practice, and teaching. A current problem in the planning of this study is the selection from among a multitude of variables of those which may be most fruitful to study. Some guidance in this matter is

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being obtained by intensive interviews of research psychologists in various fields of psychology and in different parts of the country.

A comparison of the characteristics of productive and less productive research psychologists requires the identification of persons belonging in each group. This has been done by a sort of successive-hurdles method. First, a roster of all APA members receiving doctoral degrees in the interval 1930–1944 was obtained, and a count of all *Psychological Abstracts* citations was made for each name. For each five-year period, the top 150 persons in the distribution of citation counts were selected as "high producers" in psychology. These names were submitted, along with the remainder of the roster of psychologists, to 22 judges (editors of APA journals, members of the Policy and Planning Board of the APA, and members of the Project B committee). These judges nominated persons from the remainder of the list who, in their estimation, belonged in the high-producer group. Any person who received at least two votes from these 22 judges was added to the high-producer list. This new list we might call a "highly visible" list. Size of this list for each five-year period is shown in Table 1.

For most of the persons on the "highly visible" list, or on the total roster of names, 1951 APA Directory questionnaires were available. Some preliminary results in the comparison of these groups may be of interest. These are presented in Table 2. The data in Table 2 are necessarily incomplete, since, out of the 2,000 or so names on our roster of PhD's, there were 508 who had not returned the 1951 Directory questionnaire. The project has mailed to these nonreturners a shortened form of this questionnaire, asking that it be filled out as of

TABLE 1

SIZES OF GROUPS USED IN RESEARCH PSYCHOLOGIST STUDY

Year of Degree	Total Number of PhD's		"Highly Visible"
	APA Members	Non-APA Members*	
1930–34	598	199	209
1935–39	673	165	220
1940–44	735	151	201
Total	2,006†	515	630

* These numbers are provided by colleges and universities, and undoubtedly include persons who do not call themselves psychologists. Also included are 30 persons known to be deceased.
† Omitted are four names of persons intimately associated with the project, which, if included, would make a total of 2,010.

TABLE 2

COMPARISON OF "HIGHLY VISIBLE" PSYCHOLOGISTS WITH THEIR CONTEMPORARIES

Item	"Highly Visible"	Not "Highly Visible"
Returned 1951 Directory questionnaire	540	958
Did not return questionnaire	90	418
Mean age at degree		
1930–1934 PhD's	28.1	30.8
1935–1939 PhD's	28.1	30.9
1940–1944 PhD's	28.1	31.9
Type of APA membership		
1930–1934 PhD's: Fellows	161	128
Associates	14	134
1935–1939 PhD's: Fellows	152	131
Associates	34	177
1940–1944 PhD's: Fellows	120	137
Associates	58	250
Median income, 1951		
1930–1934 PhD's	$9,286	$6,710
1935–1939 PhD's	$9,000	$6,940
1940–1944 PhD's	$8,270	$6,900

the spring of 1951, and returned, in order that we may have for each member of our sample basic biographical data. Already better than 75 per cent have been returned.

Lists of the names of the "highly visible" psychologists in each of the three five-year periods have been prepared. These lists have been sent to all persons whose names appear on them, with the instruction that the recipient is to nominate not more than 25 psychologists who, in his opinion, have made the most significant contribution to psychology as a science, either through their own research, or in their work in training PhD's in psychology. These persons were also asked to select one of five areas in psychology as the one with which they were most familiar, and to select the best 10 persons in that area, in terms of their research contribution in this area of psychology. These five areas were: Social and Personality; Experimental, Comparative, and Physiological; Clinical; Educational and Developmental; and Industrial and Vocational. This additional nominating was requested lest there be omitted from our sample any persons who had made a notable research contribution which is well recognized in one area of psychology, but which tends to be overlooked by psychologists in other areas.

These rosters have been mailed, and returns have been received from almost all persons. The analysis of votes has not reached a point, at this time, which permits the selection of the final sample of highly productive research psychologists. It is gratifying to note, however, that there is sufficient agreement among raters to make the distribution of votes markedly skewed, with several persons receiving nearly 100 per cent of the possible vote. The intention of the project is not to publish the roster of names of persons who are identified in this manner, but rather to use them as a group to study the personal and environmental factors which made them highly productive scientists.

The securing of nominations of psychologists across the country is a highly expensive and time-consuming venture, and one which cannot easily be reproduced whenever one desires to assay the worth of an individual. This project has been interested in various other ways of indicating the research competence and reputation of an individual. One of these, already mentioned, is the count of *Psychological Abstracts* citations. Another is a count of the number of times an individual's work is cited by other research workers. Through the use of IBM equipment, it is possible, without undue expense, to determine the number of times an individual's works are cited in articles of a research nature written in various journals. Some of these counts have been completed. Of some interest to the reader may be the lists shown in Table 3, indicating those persons who "lead the pack" in numbers of times their writings are cited. The number of journals on which these counts have been completed is small, and obviously unrepresentative. It is in-

tended that a complete coverage of APA and related psychological journals be obtained, in order that the total count of citations may be compared with the number of votes received from one's colleagues, as measures of research reputation.

Surveys of Jobs in Psychology in Various Communities

A major defect of studies of psychologists who are APA members is the neglect of the presumed large number of persons performing psychological services who are not affiliated with the national organization. To provide some information about such persons and to permit an on-the-spot study of environmental factors influencing psychological services and research, a series of community surveys is planned. The first of these studies has been completed, using as the survey point the County of Milwaukee, Wisconsin. Sadie Shellow conducted this survey. A second community, Providence, Rhode Island, is being surveyed currently, with Genevieve O. Rogge in charge.

The experience in Milwaukee County with this sort of survey indicated its feasibility, and provided enough information unknown to local psychologists to suggest the desirability of similar studies in other areas. Many unaffiliated and heretofore unidentified persons were found to be performing work which was in considerable part psychological in nature. In addition to the 54 APA members in Milwaukee County, 160 additional persons were found, of whom 157 were interviewed by Dr. Shellow. Local leaders in schools, government, and industry were highly cooperative in helping to find these persons, and these persons, in turn, were uniformly willing to be interviewed, and to indicate the nature of their duties, their training, and other items of relevance to the study. Thus far, the experience in Providence parallels that in Milwaukee.

It is not intended that a detailed report of Milwaukee findings be made until survey results from other communities with different characteristics are available. It is interesting to note in passing, however, that the modal degree in Milwaukee is an MA, that only about half of the persons performing psychological services received their major college training in psychology, and that the locus of employment in which the level of training is lowest is in industry.

TABLE 3

PERSONS WHOSE WORKS ARE MOST FREQUENTLY CITED, FOR FOUR JOURNALS (For the years 1949 to 1952, inclusive)

J. abnorm. soc. Psychol.	Educ. Psychol. Measmt	J. appl. Psychol.	J. exp. Psychol.
Frenkel-Brunswik	Guilford	Lawshe	Hull
Bruner	Hathaway	Guilford	Spence
Freud	Thurstone	Thurstone	Hovland
G. Allport	Kuder	Flesch	Hilgard
Lewin	Rogers	Strong	Mowrer
Postman	Bordin	Paterson	Humphreys
Sears	Guttman	Tiffin	Neal Miller
G. Murphy	Williamson	Hathaway	Tolman
	Terman		Melton

Follow-Up of BA's and MA's in Psychology

A more complete understanding of the current resources for research and service in psychology requires more information on the nature of the total potential than is now available. If, for example, we believe that too few highly qualified persons obtain PhD's in psychology, we would need to know what happens to those who show promise at the BA or MA level, but who fail to remain in training and receive the PhD. Some summary of their current activities, their attitudes and values, especially about psychology and psychological training, may yield useful insights into problems of selection and training of persons for service or research in psychology.

Currently, such follow-up studies using mail questionnaire methods are being started at Brown and Wayne Universities—two schools with markedly different traditions in psychology. These studies are only in a limited sense pilot studies, since they capitalize on the prior work of Dael Wolfle and the Commission on Human Resources and Advanced Training at The Ohio State University and the University of Michigan. Additional schools of varying traditions are being selected for study as the two current surveys get under way. Some of the additional schools whose cooperation has been obtained are: Oberlin College, Brooklyn College, the University of Minnesota, and the University of Oregon.

A Comparative Study of Fellows in Various APA Divisions

This study will concern itself with many of the variables described under the section on a Study of Research Psychologists, but will sample a more nearly representative cross section of mature psychologists, since it will confine itself neither to doctoral psychologists nor to persons receiving their degrees in a given time interval. One purpose of this study is to give insight into the differences and similarities of psychologists who identify themselves with the various areas of psychology, as these are related to date of entrance into psychology as a profession, the nature of training received, and current locus and nature of employment.

A Study of Recent PhD's in Psychology

This study will also use many of the variables used in the other studies but, by using the population of psychologists who received doctoral degrees in 1950, will permit some estimation of the nature of change in psychology or psychologists in the postwar period, in the event any changes have occurred. Changes are defined broadly to include not only differences in the personal characteristics of persons attracted to psychology, but also changes in the setting for graduate study, nature of the first job after receipt of degree, and factors influencing research productivity.

Received January 17, 1954.

THE MASTER'S DEGREE IN PSYCHOLOGY

Some Issues and Trends Discussed in Regional Conferences Relating to Subdoctoral Education [1]

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American Psychological Association

WHAT should be the nature and purpose of the master's degree in psychology? That question with many ramifications led to the creation of the Committee on Subdoctoral Education of the Education and Training Board. This Committee undertook to stimulate discussion of these problems at the grass roots level by the psychologists offering training at the subdoctoral

level; therefore, conferences on the master's degree were held in connection with each of the five regional association meetings during each of the past two years. This is a summary of the issues and trends reflected in the reports on these ten meetings.

There seems to be general recognition that the master's degree programs "fall into at least three categories: (*a*) programs of a general theoretical nature, designed primarily for students planning to continue work toward the PhD; (*b*) programs of a

general theoretical nature designed primarily for students aiming at the PhD but providing, by means of additional courses, some professional training for the student who terminates his graduate work at the master's degree level; and (c) programs oriented primarily toward professional training of various types."

The more traditional program is the first one in which the master's is the basis for the PhD in psychology. It was "apparent that the MA in nearly all eastern psychology departments is conceived as a truncated portion of the PhD program." Those who get the MA are either (a) expected to go on and get the PhD elsewhere if the institution gives only the master's degree, or (b) not allowed to go on but get the master's degree as a "consolation." This concept of the master's degree received considerable support also in the discussions and practices reported at the Midwestern meetings. There were exceptions, however, to be noted later.

The second type of program which attempts to combine theoretical education with some professional training at the master's level is, perhaps, more common in the state colleges and universities. In some instances it requires two years, and for this the degree is usually a Master of Science.

Programs of the third type aimed primarily toward professional training for a terminal master's degree were reported more common in the Southern and Rocky Mountain regional meetings, and to some extent in the Midwestern meeting. Among the special fields in which professional training was said to be available were clinical, special education, guidance, child development, and industrial. A few universities in the east trained industrial psychologists at the master's level because the demand for them is considerable. However, in the eastern institutions, there was no thought that the MA should be terminal for anyone working in the clinical field.

"In general, there was adherence to the principle of a core curriculum, pursued as a first step toward the PhD or pursued prior to, or concurrently with, professionally oriented courses. In one institution the students were said to be 'introduced to practical problems first' and later given courses in theory. The representative of another institution suggested that an alternate to a core curriculum might be the passing of all sections of a comprehensive examination—one representative wondered how much transfer there was from experimental to clinical work, and what kind of theoretical training was most

valuable for the clinician. Another question raised was that of the value of the thesis, as compared with additional course work, for students trained in clinical work at the master's degree level." Most representatives favored retention of the thesis.

In some of the conferences it was sooner or later recognized that there were two distinct problems being discussed, and each of these could be broken down into specific questions. These problems are:

a. What are the goals of university psychology departments where the master's degree is concerned, and what should be the nature of the preparation for the degree?

b. What jobs are there in the field of psychology needing persons who are trained at subdoctoral levels, and what should be the nature of this training?

It is usually assumed that subdoctoral education should involve the master's degree; however, when the two problems stated above are separated, it appears that the training of subdoctoral service workers can be considered apart from the requirements of an academic degree. In the discussions, nevertheless, a statement on one problem often involved an attitude on the other. There were marked differences of opinion expressed from the following report on one meeting:

"Several persons were apparently emphatically of the opinion that university psychology departments should give only training in theory and research. Technical training, they stated, should *not* be included as part of a university program, but should be given under the auspices of 'technical schools.' In other words, the MA should represent a scholarly degree and its holder should not feel prepared for any vocational outlet. In the words of one department representative, 'the MA degree should prepare a student for learning and not for a job.' Another person stated that this viewpoint was unrealistic and that university departments of psychology have legitimate functions other than the preparation and training of scientists. He thought that many individuals who are not psychologists but have jobs in which they deal with people should be psychologically oriented. He suggested that scholarly training does not necessarily lead to proficiency in the applied fields, and that psychology departments have definite responsibilities for providing service courses." In a different regional meeting, concern was expressed lest psychologists permit schools of education to take over the training of school psychologists. In some states,

a school district can receive aid or reimbursement for a special teacher or social worker, but not for a school psychologist. In a third different meeting, also, it was pointed out that if persons were not trained in psychology departments for certain jobs, it does not follow that this psychological work would not be done, but other service workers with less training would move in.

Assuming that psychological service workers are to be trained at the subdoctoral level, there are still the questions of what this training should be, what titles should be applied to those who complete it, and how such individuals should be identified or accredited. Some representatives suggested that adequate technical training for some applied work might even be included in an undergraduate program, but other spokesmen insisted that there should be a combination of theoretical and practical training requiring two years beyond the bachelor's degree.

For distinguishing titles, members of the Committee on Subdoctoral Education have suggested four, namely: psychological counselors, psychological examiners, psychological educators, and psychotechnologists. There was far from complete agreement or enthusiasm for these titles or for any other titles suggested. A survey reported by Q. F. Curtis in the Midwestern meeting indicated "that all the MA's felt strongly they should have the title 'psychologist,' and resisted such titles as 'service worker,' or similar ones. Others, however, feel strongly a need to distinguish between the MA and PhD psychologists for the protection of the employer." Evidently the function and status of these workers have not crystallized to the point of earning them recognized titles.

There was considerable concern that these technically trained persons might go out and try to do more than they were qualified to do without supervision. The following suggestions were made as to how control might be exercised: (a) train subdoctoral psychologists in the awareness of their limitations and responsibilities; (b) avoid using the term "psychological" in the titles of these persons, as the medical profession has avoided the term "medical" in its adjunct professions; (c) state certification laws restricting the title "psychologist"; (d) national examinations, analogous to the National Teacher Examinations.

The interest and lively discussions in the 1952 regional conferences led to recommendations that similar conferences be held with the regional as-

sociation meetings in 1953. Again, in the 1953 meetings there was considerable interest but also considerable difference of opinion. There were some indications that there was more recognition of a need at present for workers trained in psychology at subdoctoral levels, but more concern about the quality of their training. In the Southern Society meeting, six of the thirteen departments represented had a graduate core curriculum. As a prerequisite to graduate work or as a deficiency to be made up, twelve of the institutions reported that they require a course in experimental psychology, and ten that they require a course in statistics. In the Midwestern meeting, there was the statement that "regulation should be at the level of certification, rather than degree granting."

Conclusions or even a brief summary is probably hazardous, but an attempt here may stimulate the reader to formulate his own.

1. There are many questions or issues, rather than just one, related to subdoctoral education. These may be grouped under two general problems: (a) the purpose and nature of the master's degree, and (b) the jobs for service workers at the subdoctoral level and the nature of training in psychology needed for them.

2. There are wide differences of opinion on what should be the nature of the master's degree and the functions of persons trained in psychology at the subdoctoral level. There is possibly more agreement in actual practice, for the great majority of programs require a common core of experimental psychology and statistics.

3. There seems to be considerable recognition of a need for service workers in psychology at the subdoctoral level, but there is no agreement on what should be the title of these service workers.

4. For both the traditional master's degree and the more practical subdoctoral programs a thesis is generally required.

5. There is recognition of the dangers of unsupervised practice by persons with subdoctoral training, but no generally accepted proposal for controlling such practice.

The reports, with various suggestions and recommendations, from which this is abstracted, have been reviewed by the Committee on Subdoctoral Education and by the Education and Training Board, but have not been officially adopted. Any conclusions or recommendations from the Committee or from the Board are contained in their official reports.

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Ridicule in Psychological Literature

I have become concerned in recent years about the seemingly increasing use of ridicule and sarcasm in the official psychological literature. This practice flourishes particularly in theoretical articles, rejoinders to same, book reviews, and symposia. Frequently it borders on personal abuse and vilification. Typical of this brand of controversy are the exchanges that take place between psychoanalytic and nonpsychoanalytic personality theorists and between reinforcement and nonreinforcement learning theorists.

It seems to me that the use of ridicule and sarcasm in scientific publications proves nothing except the scientific immaturity and irresponsibility of the user. Certainly it brings us no closer to the solution of the theoretical problems under discussion. Furthermore, it creates a climate in which genuine communication becomes impossible and concern for truth becomes subordinated to the need for personal vindication. In such a climate graduate students can hardly be expected to acquire much respect for the dignity of theoretical inquiry.

I think that one of the chief factors responsible for the growth of this practice is the attitude of amused tolerance which many psychologists exhibit when it is employed. It undoubtedly does provide a certain amount of entertainment and comic relief. But I believe that psychology as a science would advance more rapidly if psychologists sought their amusement in movies, radio, and television rather than in scientific meetings and publications. Editors of journals and chairmen of symposia could also help in discouraging this unfortunate trend by adopting more rigorous criteria regarding the lower limits of acceptability in such matters. This could easily be accomplished without imposing any limitations upon legitimate expression of critical opinion reflective of dignified consideration of the relevant issues under discussion.

<div align="right">

DAVID P. AUSUBEL
University of Illinois

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On Editing a Text

A little over five years ago I began work on editing a text. It is now being published. During these years I have learned, often to my sorrow, that in editing a book a variety of factors are important. Not being of the school of thought that believes that experience alone teaches, I would have appreciated discovering in the literature something concerning the problems I was to encounter. I should mention that some three years ago I received an *Author's Manual* from the publisher. This manual, however, either to avoid offending by talking down or through oversight, omitted many of the following points:

1. Deadlines should be definite. In the present instance one potential contributor asked for repeated extensions on his deadline. These were granted but the manuscript was never sent in. This one person delayed the book's appearance well over a year.

2. The contributor should be asked to send in two copies of his manuscript. Suggestions and corrections can then be made on both copies and one sent back for discussion of possible changes. This was found to be more efficient than other procedures which had been tried.

3. It was found that reading a manuscript for "good English" was not enough—errors in grammar could still remain, e.g., the following sentence was allowed: "A variety of historical circumstances have all contributed. . . ." Later in galley proof the "have" had to be changed to "has" and the word "all" deleted. In the third sentence of the first paragraph of this paper a mistake of this kind was deliberately introduced to indicate how easy it is (or so it is believed) to fail to note an error of this type.

4. It was found to be of value first to look over the entire set of galleys for any queries from the publishers. In this way, permissions which were lacking could be secured, incomplete references completed, etc. without loss of time. In the more detailed work of proofreading it was found that the assistance of a nonpsychologist who was less likely to have a psychological set to see certain words or phrases was helpful. Having the same individual help in reading all the proof insured uniformity in changes made.

5. The use of italics or quotes for important words or phrases should be decided before the text is written. Related to this is the question of when to spell out numbers or when to present them as integers. Or again, one might wonder whether certain words in a journal reference should be in small print (see APA Publication Manual) regardless of whether used for purposes of reference or to acknowledge permission to reprint a graph or figure. The writer is not advocating one procedure over the other but merely suggesting consistency.

6. Finally with regard to indexing, it was found important that the following points be kept in mind: (*a*) References should be included only where one wants a student or colleague to find the particular item in question. (*b*) Other possible headings that might be used for the same material should be considered. (*c*) Indexing is often very subjective and depends upon what the author or editor thinks is important.

These points are included with the idea that they may be of value to future authors and editors.

F. L. MARCUSE
The State College of Washington

An Open Letter to Academic Psychologists

I have recently joined your ranks. In spite of my inexperience, or perhaps because of it, there are some general comments I should like to make concerning our task.

I believe current psychological knowledge in the general area of human learning can, and should, be more widely employed (*a*) directly, to improve the subject matter and techniques, and (*b*) indirectly, to clarify the goals of higher education.

I believe there is a readily demonstrable overemphasis on discrete factual data and on methodology. Both parts of this overemphasis can be seen in the nature of many of our examinations. With the aid of modern statistical devices, we are becoming very effective at estimating the quantity of information the student has collected. In some cases we are able to equate quality with predictability. But one drawback is that some of our finest students, as well as the vast group accompanying them, are developing the habit of inhaling information with the major purpose of spewing it forth, relatively unchanged, on short-answer tests. Briefly, I am most impressed with students' lack of interest in learning to think.

The best students are, by definition, sudden death on objective material. Just tell them—and they beg to be told—what detailed information is required for the course. They memorize it, arrange it on easily scored examination sheets that can be corrected by the hundreds in a few hours, and move on to more information, good grades, and, at last, the coveted label of a degree.

We seem to be in imminent danger of developing a rather cumbersome collection of card-catalogue geniuses. I even suspect that, at times, this test orientation is at least partially responsible for postgraduate work which is more an expression of an immature scramble for rubber-stamp prestige than it is an earnest striving for knowledge (functional automony?).

In any event, it appears that the only thing that saves us from chaos is the orderliness of the human nervous system. There is ample evidence in the literature of the encyclopaedic nature of much recent endeavor. While surveying this evidence we also encounter a disheartening profusion of relatively unimportant material.

This surely raises at least two questions—first, who decides what is important? I reply that *each* of us does—thus we differ, discuss, discard, and create. But then, question two—isn't this precisely what we are doing? I say yes, emphatically to the differing and discussing and more hesitantly to the discarding and creating.

My major point is that by the time students emerge from our mill of systematization and generalized fact-grubbing, all but a very few will have lost the flexible vitality and creative insight without which any art or science is doomed. As you review the course of your own higher education, are you impressed with the manner in which you were introduced to the path of mature scholarship?

Clearly, the positive step to be taken is not in the direction of intellectual anarchy, but, rather, in the direction of altering our present emphasis, altering it on the basis of present psychological knowledge.

At the start, I suggested that some of the principles of human learning that, heretofore, have only cluttered the textbooks could be applied. Might we resolve the following:

1. Instead of asking students to reproduce virtually unmodified material from text and lecture, we will phrase questions which require a *reorganization* which will, in turn, require a knowledge of *relationships* as well as the minimum amount of basic, detailed information.

2. The teaching itself will be goal oriented in terms of the *meaningful integration* of material rather than the *rote classification*.

3. In advanced scholarship we will emphasize the ends to be attained in a *larger context than immediately measurable efficiency*.

4. We will make an honest effort to select problems in terms of this larger context, rather than in terms of the method of evaluation at hand.

5. Through all the above we will recognize the importance of positive emotional concomitants in the process of learning.

I am aware of the dangers of subjective evidence, (An objective definition of "flexible vitality," or even of "larger context" is indeed a challenge.) I certainly would not encourage any sort of "censoring" of the material that already constitutes our heritage. The problem, as I see it, is one of *attitude*, stemming largely from an authoritarian educational structure that is somewhat too rigid at the upper levels. There are undoubtedly excellent students who are able to integrate large quantities of material and to think creatively, but my contention is that there are not nearly enough of them. The psychologist-educator has the means to encourage the development of these students and, conversely, to discourage the development of the grade-and-credit-oriented opportunist, and, to a lesser degree, the pedant.

LEWIS B. FRANK
*Washington and Jefferson
College*

Across the Secretary's Desk

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One thousand three hundred and sixty-seven psychologists, mostly young, became Associate members of APA on January 1, 1954. These new members have been welcomed by President Mowrer, have started to receive their "member" journals, have been sent the *Ethical Standards* and other official documents of the Association. Some of them have subscribed to "volunteer" journals, some have used the *Employment Bulletin*, some have written letters to the editor of the *American Psychologist*, some have asked the Central Office why service isn't better on January journals, some have communicated bright ideas about how APA can improve various of its operations.

The Association is 12.5 per cent larger in 1954 than in 1953. The Association is over twice as large as it was in 1948. The present Executive Secretary has acquired, in the last three years, a total of 4,997 additional and initially unexpected employers.

Each year anew many observers have made large-eyed and somewhat ambivalent comments about APA's phenomenal growth. Actually, however, the phenomenal has been routine for many years. Since our two sequential parturitions in 1892, at Clark and at Pennsylvania, the Association has doubled its membership every 7 or 8 years. Our 1954 increase of 12 per cent then is only mildly phenomenal. But in psychological arithmetic, doubling 6,000 is perhaps 18 times more impressive than doubling 1,000. At any rate, we now have 12,270. Through death and defection we will lose a few before the year is out, but in almost anybody's frame of reference we are a large organiza-

tion. And we will grow larger. Everyone is cordially invited to project Table 1 into the future.

Predicting our future growth can be a fascinating game for those whose parlors are equipped with or accessible to a calculating machine and a crystal extrapolator. Professor E. G. Boring has been for some years a devotee of this game and has played it with the recommended degree of both precision and fancy. After his most recent session he reported the following results:

A new pamphlet on my desk says that world population, 2,500,000,000 in 1950, is increasing regularly at 1 per cent per annum. There's going to be hell to pay.

The APA keeps on at 10 per cent per annum. So when will membership in the APA overtake membership in the human race? Exactly in A.D. 2100, which is 146 years from now, when both the APA and the human race will have a membership of 10,700,000,000.

So that's when to expect a shortage of psychologists that is phenomenal!

There are other contexts, more prosaic and less precise, in which we can view our increase in membership. These 1,367 new Associates will change life, in subtle but appreciable ways, for the 10,903 psychologists with whom they ally themselves. And they will change the lives of many nonpsychologists. These new members are very probably just as bright, just as well trained, just as rich in challenge, and at least as rich in opportunity as an equivalent number of psychologists who joined the Association 20 years or so ago. Will they achieve as much? Will they do as much research, write as many books, uncover just as many ideas, open just as many new fields of application, teach as many students, make as many mistakes, win as much public support for psychology? Maybe yes, maybe no. But 1,367 psychologists are not going to vegetate for 20 years. Among other things, they are going to create more psychology and more psychologists—up to the limit the culture will support.

If we were able to collect and digest existing data on social, cultural, industrial, educational, populational, and economic trends we could probably make a crude prediction about the point at which the world will become saturated with psychologists. No one has the time to collect all the data of probable relevance, and perhaps the peristaltic processes of no human being would be up to the enormous digestive chore involved. But one can find

TABLE 1

MEMBERSHIP IN APA, 1948–1954

Year	Class of Membership			Total
	Life	Fellow	Asso.	
1948	—	1,261	4,493	5,754
1949	—	1,436	5,299	6,735
1950	59	1,439	5,775	7,273
1951	69	1,506	6,979	8,554
1952	85	1,530	8,256	9,871
1953	108	1,562	9,233	10,903
1954	142	1,671	10,567	12,270

psychologists who, transported to optimism by having delivered a good lecture or discovered a significant correlation or by having imbibed other material or psychological elixers, will cast aside petty inhibitions and observe that the Century of the Psychological Man is upon us, that psychology will be asked for and be given more than we have yet conceived of. When confronted with such statements, the timid quake, the expansionists glow, the socially conscious worry, the carefree say "so what," and the cautious ask for evidence.

Whatever else we can say for or about them, psychologists are busy, creative, bright, persuasive people. It is not difficult to imagine that 1,367 of them this year and 5,000 or 10,000 more by 1960, all turned loose in the world, will represent more of a geometric than an arithmetic phenomenon. Psychologists may need to prepare for involvement with a snowball. And perhaps we had better give the world a tentative sort of caveat.

If we retreat from snow- and crystal balls to relatively grubby facts, it seems clear that psychologists will increase in number even if psychology merely keeps abreast of an increasing population. It is a fact that the birthrate in America has not yet fallen appreciably from its 1947 all-time high of 26.6 live births per 1,000 adults.[1] It is a fact that in 1951 approximately 3,898,000 children were born in this country. It is a fact that in the 4 years from 1949 through 1952, over 15,000,000 children were born, a vast majority of whom will live, barring chaos, to college age. It is a fact that for the last 50 years a steadily increasing proportion of young people in this country have gone to college; in 1900, 4 per cent of those between 18 and 21 went to college, in 1950 the figure was 28 per cent. It is a reasonable guess that by 1970, there will be 16,000,000 people between the ages of 18 and 21 and that 30 per cent of them will go to college. The enrollment then will be 4,800,000. Enrollment in 1952 was 2,148,000. In 1952 approximately 5,000 psychologists worked in colleges and universities. How many will there be in 1970?

PSYCHOLOGISTS AND THE SCIENTIFIC REGISTER

The National Science Foundation has taken steps to construct an up-to-date and continuing Register

[1] These and subsequent facts are presented in WELLE-MEYER, J. F., JR., & LERNER, PAULINE A. Higher education faculty requirements in the humanities and the social sciences, 1952–1970. *Sch. & Soc.*, 1953, **78**, 145–152.

of Scientific and Technical Personnel. The Foundation plans to use the Register data for extensive statistical studies of the country's specialized personnel. The Register will also be a source of ready information, in case of emergency, on individuals with special skills and experience.

In going about this project the Foundation has elicited the cooperation of the various scientific and technical organizations. A pattern of collaboration has been set up whereby the various organizations, with financial support and technical guidance from the Foundation, actually carry out the registering, supplying the Foundation with duplicate sets of IBM cards carrying basic information. Already the chemists, mathematicians, physicists, engineers, and others have started their projects. The APA Board of Directors recently voted that we too should participate. The National Science Foundation then made APA a grant to support the activity. Soon all members of the Association will receive a request to fill out a brief questionnaire giving the facts of training, fields of specialization, etc. It is hoped that all members will take the time to collaborate in this venture.

Those members who recall the 1951 questionnaire, also connected with a National Register, should not shudder prematurely at the prospect of receiving the forthcoming instrument. It will be not nearly so horrendous. In 1951 we attempted to secure, at one fell blow, information not only for the Register but also for the APA Directory and for the use of APA government. The forthcoming questionnaire will be focused almost exclusively on the Register and will not be at all concerned with the Directory.

One interesting aspect of the project, incidentally, is that we will attempt to get complete information about psychologists who are not members of the Association. If we succeed, we will have for the first time some decent evidence on how many psychologists there really are in the country and will know what non-APA psychologists do and where they do it. Horseback estimates have it that there are from 3,000 to 5,000 people who meet APA standards but are not members. Those who think APA is already too large may take comfort in the fact that, though some members now feel that non-member psychologists are shirking a responsibility, APA has never conducted a "membership drive."

THE 1954 DIRECTORY

Present plans, announced elsewhere in this issue, call for a June or July publication of the 1954 Directory. If our members cooperate, if the printer is realistic in his estimates and diligent in meeting them, and if our own experience with the new Flexoprint system is not misleading, our plans will be implemented. We think they will be.

Members whose entries have not changed since the last Directory was printed do not need to bother about the new Directory. They are in. All others, however, do need to cooperate—unless they already have communicated explicitly with the Directory Office. They can cooperate by filling out and mailing in the form on page 134.

The new Directory will be under the editorship of Mrs. Bette Goodrich. Jane Hildreth, who for four years has labored so hard and so well to produce our directories, has moved on to other Central Office pastures, pastures offering the greater greenness that comes, for versatile people, with greater variety. She will now spend more time on work here for the Conference of State Psychological Associations and membership matters.

THE INTERNATIONAL CONGRESS

The joint committee of the Canadian Psychological Association and of APA, under the chairmanship of Donald G. Marquis, has been very active and creative in arranging the Fourteenth International Congress of Psychology. The Congress will be held in Montreal, June 7–12, and promises to be an outstanding psychological event of our time. For many foreign psychologists the Committee has been something of a fairy godmother in raising money to support travel, in finding short-term appointments here and in Canada for visiting colleagues, in facilitating contacts and arrangements. For North Americans who will attend the Congress the Committee's role has been closer to that of a benign godfather. The program is carefully planned to offer the best available fare for the most psychologists. It will be the first meeting most of us have attended where the program is primarily for the audience rather than for those who, for one good reason or another, volunteer to present papers. It will be an interesting experiment in social engineering.

With respect to less intellectual interests also, the Committee has planned well. Those who attend will not only have good opportunity to steep in the persisting picturesqueness of Montreal but to participate fully in special social affairs, some planned, some unplanned, some involving official ceremonial, some with none but the most relaxing rituals.

The Committee has done a remarkable job. But the Committee is nervous. Or if it isn't, it should be. It is caught in a Scylla-Charybdis situation calculated to produce all manner of psychosomatic things. Its members must worry about having so many Americans come that (a) guests from abroad are inundated or (b) the facilities of Montreal are overtaxed. They must worry also lest so few Americans come that there will not be enough registration fees to meet financial commitments, e.g., the promise of travel and assistance for distinguished Europeans. APA members are about the only people who can help the Committee exchange its present nervousness for worries of a more productive sort. Those who plan to attend the Congress should *register now*. A registration form was printed in the November 1953 *American Psychologist*. Those who do not have that issue should write to Father Noël Mailloux, Institute of Psychology, University of Montreal, Montreal, P. Q., Canada.

THE *American Psychologist:* NOTES AND NEWS AND ASSOCIATION AFFAIRS

Many readers of the *American Psychologist* tell us they turn first to Psychological Notes and News, and some say this is the only part of the journal they read. Although this section does not have the juice of a gossip column, it is widely read and does serve, in some measure, to keep psychologists usefully informed about their colleagues. Because we often receive inquiries about the policies followed in editing this section, we are giving here, in brief, some answers to the more recurrent questions.

Except in rare instances, we do not print news about non-APA members, on the grounds that the journal is, as the masthead states, "the professional journal of the American Psychological Association, Inc." We do not include new addresses, simply because if we printed some of them, we would have to print them all, and all address changes made during a year would just about fill the journal's annual page allotment. In general, we print only items that come to us in writing over someone's signature. All items are subject to editorial revision to conform to the format and style

of the journal and to meet space requirements. And finally, at the present time the deadline for receipt of items is the tenth of the month preceding the month of issue.

For Notes and News, as for all other parts of the journal, we are dependent upon contributions. We have no reporters to track down our news. For this reason, we hope that all APA members will send us information about what is happening to them. We would especially like to have heads of psychology departments or other organizations send us news about their staffs and their activities.

We also want news about Association affairs. The November *American Psychologist* carries a long list of APA divisional officers, committees, and representatives to other organizations. As far as many APA members are concerned, this is probably the only sign that these groups and individuals are in existence. The committees and representatives all report annually to the Board of Directors and the Council, but unless these reports are ordered printed, they are filed away in the APA archives, and very few members know in detail what they contain. Many APA divisions get out newsletters for their members; although much of the material is very division-centered, some of it is undoubtedly of general interest, and we think the best place to bring it to light would be in the *American Psychologist*. What we would like to do is to publish brief reports, articles, news, announcements— "brief" meaning not over 1,000 words and preferably no longer than necessary to reach the ground—about any interesting activities or developments of these various APA groups. We hope they will send us such reports. We cannot promise to publish all of them, for we have to act like editors, but chances are good that if someone is motivated to write something for the journal, it will strike us, too, as worthy of publication.

THE BUILDING FUND

The October 1953 *American Psychologist* carried the names of all (or almost all, *vide infra*) individuals and organizations who had contributed in the form of payments and/or pledges to the APA Building Fund through September 30, 1953. Contributions have continued to come in. The total amount contributed on February 1 was $22,057.93, leaving us approximately $74,406.35 to go before we have reached the official goal.

The October *American Psychologist* said it contained the names of all who had contributed by September 30. It was wrong. The following should also have been listed: Wendell E. and Bernice M. Jeffrey, Arthur Kornhauser, and Psychological Research Associates.

The names of the October through January contributors are listed below.

Bernal del Riesgo, Alfonso
Bernstein, Abraham
Bogardus, Helen
Boland, Ruth F.
Brudo, Charles S.
Bryan, Beatrice Irene
Buchanan, Donald A.
Burns, Mary A.
Carlson, Wendell R.
Cash, Harold C.
Clark, Kenneth E.
Cocheres, Thomas L.
Cohen, Louis D.
Combs, Arthur W.
Conners, George H.
Detambel, Marvin H.
Diamond, A. Leonard
Eldred, Donald M.
Fenton, Norman
Fernandez, Elvira J.
Fisher, Jerome
Fochtman, Vincent A.
Fortier, Robert H.
Fosberg, Irving A.
French, Robert L.
Furst, Edward J.
Giedt, F. Harold
Gladstone, Roy
Grimes, F. Vincent
Grings, William W.
Hall, Calvin S.
Hamilton, Charles L.
Hansen, C. Frederick
Harlow, Harry F.
Heisler, Verda T.
Henry, Edwin R.
Hibler, Francis W.
Hitchcock, Arthur A.
House, Joseph W.
Houston, Robert C.
Iverson, Marvin A.
Jonas, Richard O.
Kelso, Duane F.
Klass, Bertrand
Klemmer, Edmund T.
Landreth, Catherine
Lavender, H. Jerry, Jr.
Lawshe, C. H.

Levine, Louis S.
Linneman, Jessica
Macfarlane, Thomas G.
Marley, Francis W.
Marx, Melvin H.
Maxfield, Kathryn E.
May, Rollo
McKenna, Frank S.
Meltzer, Hyman
Mercer, Mary L.
Mertens, Marjorie S.
Mueller, A. D.
Park, James J.
Porter, Paul B.
Pratt, Carroll C.
Quaytman, Wilfred
Replogle, Fred A.
Robertson, Marjorie B.
Rodgers, David A.
Rohrer, Perry L.
Rose, Nicholas
Sanderson, J. Wesley
Sanderson, Sidney
Sands, Harry
Saupe, Mildred W.
Schnadt, Frederick
Schonbar, Rosalea Ann
Smock, Charles D.
Starer, Emanuel D.
Sward, Keith
Taylor, William S.
Terry, Paul W.
Thomson, Calvin W.
Uhrbrock, Richard S. &
 Esther Gatewood
Weldon, Roger J.
Whiteman, Martin &
 Dorit B.
Wiggins, Jack G.
Williams, Gertha
Zucker, Herbert
Cleveland Psychological Association
Psi Chi (Loyola Chapter)
Puget Sound Psychological Association

Psychological Notes and News

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Garry R. Austin, counselor and assistant professor at the Counseling Center, Michigan State College, was killed in an automobile accident on January 29.

Bryant F. Nagle died January 4, 1954 of meningococcic meningitis at the age of twenty-four. He received his PhD degree from Purdue University in May, 1953.

H. B. English represented the APA at the meeting of the first Brazilian Congress of Psychology and conveyed the greetings of the Association. Besides presiding at one of the sessions, Dr. English read a paper entitled "Uma Teoria da Emoção."

William M. Smith has just returned from Belgium after having spent the past term studying with Professor Albert Michotte of the University of Louvain. His stay was sponsored by the Fulbright Program with an appointment as a research scholar. He has now returned to his duties with the psychology department of Princeton University.

Earl S. Taulbee, assistant chief psychologist at the Norfolk (Nebraska) State Hospital, has received an appointment to the faculty of the University of Nebraska College of Medicine with the rank of clinical assistant in medical psychology.

Frederick S. Hauser has been appointed assistant professor of psychology and director of the child study program at Hollins College, Virginia.

Kenneth E. Moyer, assistant professor of psychology at Carnegie Institute of Technology, has been given a leave of absence for the current semester for an assignment in Norway. He will assist in instituting an undergraduate program of industrial psychology into the curriculum of the Norwegian Institute of Technology. The program will be similar to that given in the College of Engineering and Science at Carnegie.

William R. Reevy, formerly an assistant to Clifford R. Adams in the Marriage Counseling Service at Pennsylvania State University and a graduate of the VA Training Program of that University, is now a Grant Fellow in the Marriage Counseling Service and Training Program at The Menninger Foundation, Topeka, Kansas.

Jack R. Danielson, formerly clinical psychologist at Ford Ord Army Hospital, is now teaching at Napa High School and Napa Junior College, Napa, California.

Charles A. Ullmann has been appointed director, Career Development Programs, for the U. S. Civil Service Commission. He was formerly clinical psychologist for the USPHS Mental Health Clinic at College Park, Maryland.

Theodore C. Kahn has been transferred from the Neuropsychiatric Service, USAF Hospital, Parks Air Force Base, California, to the staff of the School of Aviation Medicine, Randolph Air Force Base, Texas, where he will teach clinical psychology to the medical officers who attend the school.

Harry Waller Daniels has moved to Caracas, Venezuela, to take a position as head of the program development subsection of the Training Section for Creole Petroleum Corporation. Mr. Daniels was previously associated with Richardson, Bellows, Henry & Company as a project manager.

Arthur Mann, formerly a specialized counselor with parolees for the New York State Department of Labor, has been appointed assistant to Robert R. Hannum, Director of Vocational Placement. Mr. Mann had previously been employed as a clinical psychologist by the Department of Mental Hygiene.

Robert P. Holston, formerly with Richardson, Bellows, Henry & Company, has joined the staff of Management Development Associates, New York City.

Newly added to the psychological staff of the University of Maryland Medical School, Hospital, and Psychiatric Institute are: **Lester M. Libo,** formerly of the University of Michigan, assistant professor and head of medical psychology; **Benjamin Pope,** formerly of Spring Grove State Hospital, instructor; and **Thomas D. Haupt,** formerly of Catholic University, assistant. Associated with the interdisciplinary research project on poliomyelitis are **Harvey A. Robinson,** formerly of Yale University, research associate and project coordinator,

and **Arthur Silverstein,** formerly of New York University, research assistant.

Phyllis J. DeLano, formerly of the Chrysler Corporation, has been appointed resident counselor at the University of Cincinnati, College of Nursing and Health.

David Kipnis is no longer with the consulting firm of Richardson, Bellows, Henry & Company. At present he has received a grant from Memorial Center for Cancer and Allied Diseases to conduct a record survey of the needs of advanced cancer patients.

Stanley R. Ostrom, formerly coordinator of child welfare services, Long Beach (California) City Schools, has been appointed to the position of director of test development, and **William M. Shanner,** formerly executive director, University of Oklahoma Research Institute, has been appointed to the position of director of professional services at the California Test Bureau.

Myron H. Gordon, formerly of the Queens College Educational Clinic, is now in private practice in Queens, New York. He is also a consultant for the Windham Children's Service, Children's Day and Night Shelter, Queens College Speech and Hearing Center, and Flushing Hospital.

Virginia N. Kerr, formerly of Buffalo State Hospital is now clinical psychologist on the staff of the Family Service Society, Buffalo, New York.

The University of California at Berkeley announces that for the Summer Session of 1954, **J. S. Bruner,** Harvard University; **H. J. Eysenck,** University of London; **H. F. Harlow,** University of Wisconsin; and **D. O. Hebb,** McGill University, will join its regular staff. Professors Eysenck and Hebb will be in residence June 21 to July 31, and Professors Bruner and Harlow from August 2 to September 11.

Henry L. Pope has recently joined the staff of Bruce Payne & Associates, Management Consultants, Westport, Connecticut, as a senior associate.

At the University of North Dakota **W. Lynn Smith,** assistant professor and clinical psychologist has resigned in order to take the position of supervisor of clinical and allied services at the State Hospital at Jamestown, North Dakota. While at the State Hospital he will, however, retain his rank in the university department of psychology, supervise a program for clinical interns, and conduct a

clinical seminar. **Daniel G. Brown,** formerly at Texas College of Arts and Industries, has been appointed assistant professor and clinical psychologist. Other members of the department of psychology are **Daryle E. Keefer,** professor and dean of the graduate division; **Hermann F. Buegel,** associate professor and acting head of the department; **Guy E. Brown, Jr.,** assistant professor and director of counseling services; and **James D. Mathisen,** assistant professor and state supervisor of guidance services.

The **U. S. Naval Personnel Research Field Activity,** San Diego, California, is engaged in applied personnel research in the general areas of selection and classification, training, organizational analysis and manpower utilization, and occupational analysis. APA members on the staff of the Classification Research Department are Leonard V. Gordon, director; Adolph A. V. Anderson, Kenneth W. Eells, Alfred F. Hertzka, Bernard Rimland, and Leonard C. Swanson. Members of the Training Research Department are Earl I. Jones, director, and Robert L. Batterton, Chester R. Bilinski, James S. Ford, Herman Roemmich, and A. John Stauffer. Barry Fagin (on leave from the University of Florida) is director of the Occupational Research Department and other members are Albert B. Chalupsky and Norman Friedman. Warren Torgerson (at present on active duty as Lt., USNR) is acting director of the Statistical Department and Edmund E. Dudek is chief scientist of the Activity.

The **Norwalk Psychiatric Clinic,** Norwalk, Connecticut, has greatly expanded its services to both adults and children since the Clinic moved to new quarters in the Norwalk Hospital last fall. James R. Dickenson is full-time director of the Clinic; Helen Reed Thompson is clinical psychologist; Helen C. Bonime is psychiatric social worker.

The Psychology Laboratory of the Institute for Psychosomatic and Psychiatric Research and Training of Michael Reese Hospital announces the addition of the following to its staff: Irving M. Goldstein, clinical psychologist; Jacques A. Chevalier and Philip Lichtenberg, research psychologists. Seymour Levine has been appointed as United States Public Health Service postdoctoral fellow in the program jointly sponsored by the University of Illinois Medical School, the University of Chicago, Northwestern University, and Michael Reese Hos-

pital. He is stationed this year at Michael Reese Hospital.

VA DEPARTMENT OF MEDICINE AND SURGERY ANNOUNCEMENTS

Clinical Psychology

Helen S. Ammons, a graduate of the VA Training Program, Tulane University, has been appointed to the staff of VA Center, Wood, Wisconsin.

Robert W. Bauer, a graduate of the VA Training Program, University of Chicago, has been appointed to the staff of VA Hospital, Sheridan, Wyoming.

Norman Berk, VA Hospital, Northport, L. I., New York, was erroneously listed in the roster as Norman Beck.

Theodore E. Burik, a former VA trainee at Fordham University, has been appointed to the staff at VA Office, Trenton, New Jersey.

Harold Bessell has been appointed to the staff of VA Center, Wichita, Kansas.

Herman Y. Efron has been appointed to the staff of VA Hospital, Louisville, Kentucky.

William Hallow has transferred from VA Hospital, Topeka, Kansas, to the position of Chief Clinical Psychologist, VA Hospital, Lebanon, Pennsylvania.

Murray Levine, who was listed in the roster at VA Hospital, Pittsburgh, Pennsylvania, has accepted a position at VA Regional Office, Philadelphia, Pennsylvania.

Richard G. Murney has transferred from VA Hospital, Kecoughtan, Virginia, to VA Hospital, Fort Lyon, Colorado.

Mary Martha Murphy has been appointed to the staff of VA Center, Bath, New York.

Ann F. Neel, a graduate of the VA Training Program, University of Michigan, has been appointed to the staff of VA Hospital, Kansas City, Missouri.

Frances Racusen has transferred from VA Hospital, Downey, Illinois, to VA Hospital, Chicago, Illinois.

Henry N. Riccuiti was listed in the roster at VA Hospital, Brockton, Massachusetts, but declined appointment.

Gideon B. Stone has been appointed Chief Clinical Psychologist at VA Hospital, Perry Point, Maryland.

Charles Taffel has transferred from VA Regional Office, New York, New York, to VA Hospital, Brockton, Massachusetts.

J. Arthur Waites has transferred from VA Hospital, Perry Point, Maryland, to the position of Chief, Clinical Psychology Training Unit, VA Center, Los Angeles, California.

Glenn M. Woolf has been appointed to the staff of VA Hospital, Houston, Texas.

Counseling Psychology

Grady Eugene Carson, formerly a psychologist with the Texas State Prison, Huntsville, Texas, has been appointed to the staff, Vocational Counseling Service, VA Hospital, Waco, Texas.

W. Frank Caston, a graduate of Vanderbilt University, has been appointed Chief, Vocational Counseling Service, VA Hospital, Augusta, Georgia.

William A. Hunter, formerly with the Los Angeles VA Regional Office, has been appointed to the staff of VA Hospital, Long Beach, California.

Wirt M. Wolff, a graduate of the VA Training Program, Stanford University, has been appointed Chief, Vocational Counseling Service, VA Hospital, Palo Alto, California.

Psi Chi has announced that the Britt Foundation's Grant-in-Aid for 1953 has been awarded to Vincent F. O'Connell of Adelphi College for his research on "A technique and an electronic device for the evaluation and analysis of voice quality in schizophrenia." The chairman of the Psi Chi Committee on Awards is Max Meenes, Howard University. This is the fourth annual award made through Psi Chi by the Britt Foundation. The grant provides assistance for research by graduate students. It is hoped that a similar grant may be established in the future to aid research by undergraduate students in psychology.

The First Inter-American Congress of Psychology, initiated by the Inter-American Society of Psychology and sponsored by the Dominican Government, was held from December 10–20, 1953, in Ciudad Trujillo, Dominican Republic, at the University of Santo Domingo, oldest university of the Americas. President of the Congress was Andres Avelino, University of Santo Domingo; the vice-president was Werner Wolff, Bard College, and the secretary general was Oswaldo Robles. Four sessions dealt with the following problems: (a) objectives of the psychological disciplines; (b) training and professional tools; (c) psychology as to national and international values; (d) psychology and ethics.

The meetings and discussions led to the formation of the following committees: (a) a Commit-

tee on Publications, to edit the transactions and discussions of the Congress and to project an inter-American communication bulletin as a bilingual publication; (b) a Coordinating Committee for the exchange of professors, students, films, and psychological material; (c) a Committee for the Study and Unification of Inter-American Research, to unify the definition of psychological terms, to study cultural deviants in tests, and to advance the publication and evaluation of psychological methods; (d) a Committee for the Study of Thought Patterns in the Americas, to bridge the experimental and philosophical aspects of psychology through a systematic study of concepts in different age and social groups. The following new officers of the Inter-American Society of Psychology were elected: President, Oliver Brachfeld, University of the Andes, Merida, Venezuela; vice-president, Willard C. Olson, University of Michigan; secretary general, Werner Wolff, Bard College; treasurer, Gustave M. Gilbert, Michigan State College; associated vice-presidents: Herbert S. Langfeld, Julian M. Blackburn, Andres Avelino, Manuel Falcon, Victor Funes Donaire, Carlos Nassar, Emilio Majluj, Enrique Roxo, and Eduardo Krapf. For the Second Congress an invitation has been received from the representative of Venezuela. The tentative plan is to hold the meeting from December 15–22, 1954 in Caracas and Merida.

At the 1953 annual convention of the American Psychological Association a new national organization, the Society of Correctional Psychologists, was formed for psychologists actively engaged in the correctional and penal areas. It was formed by the group of psychologists who had been associated with the round table discussions on "Psychology in Prisons" (under the auspices of Division 18). Among other functions and services, the Society of Correctional Psychologists will fill the need for (a) propagation of ideas and nationwide communications of the interest group, (b) improvement of standards and status through better public relations with press and public service officials, (c) improvement of the role of treatment programs in institutions, (d) more active role in public service planning for future programs by correctional psychologists, (e) stimulation of research and exchange of data in this area. Officers are Arnold V. Goulding, president; Captain William R. Perl, first vice-president; John D. Adams, second vice-president; and Sheldon B. Peizer, secretary-treasurer.

The University of Chicago department of psychology announces two workshop seminars in the Rorschach test, July 6–10, 1954, and July 12–16, 1954, to be conducted by S. J. Beck. Workshop I, Basic Processes, will provide a grounding in fundamentals. Workshop II, Advanced Clinical Interpretation, will consider the ego, anxiety, and the individual's psychological reserves as treatment potential. Workshop I may be taken by students at, or ready for, the intern level. Admission to Workshop II is limited to psychologists and psychiatrists in clinical positions or practice. Each seminar will meet at the university for five days, with two sessions each day, two hours per session. For information as to admission, fees, or academic credit arrangements, write to the Executive Secretary, Department of Psychology, University of Chicago, Chicago 37, Illinois.

The Merrill-Palmer School has announced a 1954 summer program in human relations. There will be a seminar on the Merrill-Palmer campus in Detroit on June 21–July 31 and a workshop at the Merrill-Palmer camp on June 28–August 6. For detailed information write to the Registrar, the Merrill-Palmer School, 71 East Ferry Avenue, Detroit 2, Michigan. The Merrill-Palmer School has also announced several fellowships, assistantships, and special grants for 1954–1955. Detailed information and application blanks may be obtained from the Registrar, the Merrill-Palmer School, 71 East Ferry Avenue, Detroit 2, Michigan.

Applications are being accepted for two post-doctoral fellowships in clinical psychology at The Menninger Foundation, one offering specialization in the area of adult psychiatry, and one in child psychiatry. Both fellowships are for a two-year period and provide advanced training in diagnostic testing with both children and adults, as well as opportunity to study psychotherapy and do research. The fellowship in the area of child psychiatry is offered pending confirmation of USPHS support for this portion of the training program. Tax-free stipends of $4,100 in the first year and $4,300 in the second year will be provided. A PhD in psychology with a minimum of one year of supervised clinical experience is required. For application forms, write to Martin Mayman, PhD, Director of Psychological Training, The Menninger Foundation, Topeka, Kansas.

Convention Calendar

American Psychological Association: September 3–8, 1954; New York City
For information write to:
Dr. Fillmore H. Sanford
1333 Sixteenth Street N. W.
Washington 6, D. C.

Society of Experimental Psychologists: April 1–2, 1954; Ithaca, New York
For information write to:
Dr. James J. Gibson
Department of Psychology
Cornell University
Ithaca, New York

Illinois Psychological Association: April 3, 1954; Champaign-Urbana, Illinois
For information write to:
Dr. Stan S. Marzolf
Illinois State Normal University
Normal, Illinois

Eastern Psychological Association: April 9–10, 1954; New York City
For information write to:
Dr. G. Gorham Lane
Department of Psychology
University of Delaware
Newark, Delaware

American Personnel and Guidance Association: April 11–15, 1954; Buffalo, New York
For information write to:
Dean Gordon Klopf
State Teachers College
1300 Elmwood Avenue
Buffalo, New York

Southern Society for Philosophy and Psychology: April 15–17, 1954; Atlanta, Georgia
For information write to:
Dr. O. L. Lacey
Department of Psychology
University of Alabama
University, Alabama

West Virginia Psychological Association: April 24, 1954; Clarksburg, West Virginia
For information write to:
Dr. O. S. McLean
Veterans Administration Hospital
Clarksburg, West Virginia

International Council for Exceptional Children: April 27–May 1, 1954; Cincinnati, Ohio
For information write to:
Harley Z. Wooden, Secretary
1201 Sixteenth Street N.W.
Washington 6, D. C.

Florida Psychological Association: April 29–May 1, 1954; St. Petersburg, Florida
For information write to:
Dr. Theron Alexander
Department of Psychology
Florida State University
Tallahassee, Florida

Midwestern Psychological Association: April 29–May 1, 1954; Columbus, Ohio
For information write to:
Dr. Lee J. Cronbach
Bureau of Research and Service
University of Illinois
1007 South Wright Street
Champaign, Illinois

Ohio Psychological Association: April 30, 1954; Columbus, Ohio
For information write to:
Dr. Rosina M. Brown
Cleveland Board of Education
Cleveland 14, Ohio

Rocky Mountain Branch of the APA: April 30–May 1, 1954; Laramie, Wyoming
For information write to:
Dr. Lawrence S. Rogers
1046 Madison Street
Denver 6, Colorado

Western Psychological Association: May 20–22, 1954; Long Beach, California
For information write to:
Dr. Leona Tyler
University of Oregon
Eugene, Oregon

American Society of Group Psychotherapy and Psychodrama: May 2–3, 1954; St. Louis, Missouri
For information write to:
Dr. Edgar F. Borgatta
Laboratory of Social Relations
Harvard University
Cambridge 38, Massachusetts

American Psychiatric Association: May 3–7, 1954; St. Louis, Missouri
For information write to:
Mr. Austin M. Davies
Room 310
1270 Avenue of the Americas
New York 20, New York

Pennsylvania Psychological Association: May 14–15, 1954; Harrisburg, Pennsylvania
For information write to:
Dr. Inez M. Husted
Luzerne County Schools
Deposit and Savings Bank Building
Wilkes-Barre, Pennsylvania

American Association on Mental Deficiency: May 18–22, 1954; Atlantic City, New Jersey
For information write to:
Mr. Edward L. Johnstone
Chairman, Arrangements Committee
Woods School
Langhorne, Pennsylvania

American Psychopathological Association: June 4–5, 1954; New York City
For information write to:
Dr. Donald M. Hamilton
American Psychopathological Association
121 Westchester Avenue
White Plains, New York

Canadian Psychological Association: June 5–6, 1954; Montreal, P. Q., Canada
For information write to:
Dr. G. A. Ferguson
Peterson Hall
3600 McTavish Street
Montreal, P. Q., Canada

International Congress of Psychology: June 7–12, 1954; Montreal, P. Q., Canada
For information write to:
Professor Noël Mailloux
Institute of Psychology
University of Montreal
Montreal, P. Q., Canada

INSTRUCTIONS TO MEMBERS CONCERNING THE 1954 APA DIRECTORY

The 1954 edition of the APA Directory will be printed in June or July. It will incorporate entries for the 1954 Associates, and changes in entries appearing in the 1953 edition. *If you are an Associate elected as of January 1, 1954, or if your existing entry needs to be changed or completed, please fill out the blank below in full.* You are strongly urged to read the explanatory material that follows the blank before putting pen or typewriter to it, for the matter is more complicated than it seems at first thought. Please do *not* send in the blank if no changes have occurred in your 1953 entry, or if you have already informed the Directory Office of a change or correction. (The Directory Office does *not* receive routine changes of address unless the member mentions the Directory.)

Please print or typewrite, and complete *in full*, even though the change may affect only one item.

Explanatory Notes Concerning the 1954 Directory

In the Preface to the 1953 Directory, members were asked to keep the Directory Office informed of any and all changes that occur in name, mailing address, highest degree, and primary position held. A few of the members have done this, but because many of them have not, the present blank is printed here as a reminder and as an easy way to inform the Directory Office of changes that should be reflected in the 1954 edition. You are asked to complete the blank in full (even though the change may be in only one of the items) because the Directory Office needs to be reassured that a change in one of the items *is* or *is not* accompanied by a change in one or more of the others. Experience with previous directories indicates that a major change in one of the items is almost always accompanied by changes in the others. If the change does affect only one item, however, it would be sufficient to type "No change" in the other spaces on the blank.

The Central Office is responsible for keeping up to date the information about APA membership status, ABEPP Diplomate status, and certification and/or licensure by the states. Here are guide lines for submitting information about the other four items for which the members are responsible.

Name. In the Flexoprint directories the name of each member is given as he shows it in his signature, unless he expressly asks otherwise. No part of the name is parenthesized. By *title* we mean Dr., Mr., Mrs., Miss, etc., *not* the position title. Cross references are included for all members who have changed their names since the last Directory and, in the case of 1954 Associates, will be included for any who have changed their

names since the publication of the list in the January 1954 *American Psychologist*.

Mailing address. This is the address that will be used by the readers of the Directory. It need not be the same as the one used for APA journal mailings, and quite often it is not. For this reason, it is very important, when your address changes and you are telling APA about it, that you specify whether it is merely a change for journal mailings, or whether you wish the change made in the Directory also. If the latter, then you will need to tell the Directory Office *whether or not* the change is accompanied by a change in position, highest degree, title, or name.

Highest earned degree. Only the highest earned degree, with year and institution, is printed in the Flexoprint directories. If your degree status changes, therefore, you will need to tell us what new degree you obtained, and when and where you obtained it.

Primary position held. Only one position is given in the entries for the Flexoprint directories. If you list more than one, the Directory Office has to decide which one to print. The city address is given for the employing institution, if different from the city of the mailing address, and in the geographical directory the member is listed under his place of employment.

Changes will be made in the Directory up to the last possible minute. It is unlikely, however, that many can be made after May 15th. This is not a set deadline, but is mentioned as a help to those who are anticipating changes. The more information we get by that time the more flexible we can be in making last-minute changes.

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